PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXVIII.

PART I.

LONDON,

PRINTED BY W BULMER AND CO. CLEVELAND-ROW, ST. JAMES'S;
AND SOLD BY G. AND W. NICOL, PALL-MALL, BOOKSELLERS TO HIS MAJESTY,
AND PRINTERS TO THE ROYAL SOCIETY.

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to

2760 5

be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities, of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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ERRATA.

Page 72, under "Corr. for arc," line 2nd, *for* 1,15 *read* 1,51.
——— under "vibrations for 24 hours," line 2nd, *for* 86056,93 *read* 86057,29.
——— "Great weight above," *for* mean 86057,70, *read* 86057,79.
Page 86, Experiment B, *for* 86057,70, *read* 86057,79.
——— Experiment B, under "Diff." *for* ,23 *read* ,14.
Page 87, Experiment B, *for* 86059,93, *read* 86057,93.
Page 116, line 22, *for* "constitute," *read* "consfitutes."

PHILOSOPHICAL

TRANSACTIONS.

The PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged the Medal on SIR GODFREY COPLEY's Donation, for the year 1817, to CAPTAIN HENRY KATER, F. R. S. for his Experiments on the Pendulum.

And they adjudged the Gold and Silver Medals, on the Donation of BENJAMIN COUNT of RUMFORD, to SIR HUMPHRY DAVY, LL.D. F. R. S. for his Papers on Combustion and Flame, published in the last Volume of the Philosophical Transactions.

PHILOSOPHICAL TRANSACTIONS.

I. *On the great strength given to Ships of War by the application of Diagonal Braces.* By Robert Seppings, *Esq. F. R. S.*

Read November 27, 1817.

SINCE the time that I first suggested the principle of applying a diagonal frame-work to ships of war, which was first partially and successfully adopted in the Kent, a seventy-four gun ship, in the year 1805, my mind has been continually and anxiously turned to this important subject; and it was not until the utility of the experiment had been fully established in the opinion of most naval officers, that I ventured to present to the Royal Society, a paper on the application of this well known principle to the construction of large ships of war, but which, as far as my knowledge extends, never had before that time been so applied, either theoretically or practically, in this, or any other maritime country; and I am well assured, that no such application, or suggestion, appears in any of the Continental writers on naval architecture. I merely mention this, because it has been pretty broadly insinuated, that the idea was borrowed from the French. The propriety of a different disposition of

the materials entering into the construction of a ship, has at different times, for more than a century past, been suggested by English ship-builders; and partial alterations have, in consequence, been introduced; but no one, that I am aware of, has at any time proposed the system of a diagonal trussed frame. If I have received any assistance in the progress of this new system, now universally adopted in the British navy, it was from the plans and drawings of the celebrated bridge of Schaffhausen, and from no other source.

The extensive application of this principle to no less than thirty-eight sail of the line, and thirty frigates, might perhaps be conclusive as to the advantages expected to be derived from the new system; but as the Royal Society did me the honour to introduce my account of that system into their Transactions, at an early period of its adoption, I am led to hope that the result of a practical experiment, made with a view of proving the correctness of the principle, may not be deemed an improper or an uninteresting corollary to my former paper.

In the early part of this year (1817) the Justitia, an old Danish seventy-four gun ship, was ordered to be broken up on account of her defective state; and having observed her to be considerably arched, or hogged, as it is usually termed, I determined, notwithstanding her age and defective state, to apply the trussing principle to a certain extent, with a view to observe what effect it would produce on a fabric reduced to so weak and shaken a condition.

The officers of the yard were directed to place sights on the lower and upper gun-decks, prior to her being taken into the dock; and to ascertain, when she grounded on the

blocks, how much she had altered from the state in which she was when afloat. They were then to place a certain number of trusses (conformably with the annexed drawing, Plate I. No. 1.) in the following manner: those in the hold marked A, to be placed in an angle of 45°, or thereabouts, and those marked B, at right angles to them; those in the ports marked C to be placed from the midships forward, in an angle of about 40°, and, from the midships aft, at the same angle, but in an opposite direction. As it was uncertain where the centre of fracture (or point of separation) would take place, a few of the port-holes about the centre of the ship were crossed, as shown in Plate I. at D. Wedges were applied to the heels of the trusses, which were then set tight. The ship being thus partially trussed, the water was then to be let into the dock, and the ship floated out of it into the bason, where she was to lay one hour, when a committee was to examine the sights, and ascertain how much the ship had altered; and again, what change had taken place in twenty-four hours after floating. This being done, the trusses were to be disengaged in as short a time as possible, in order to observe whether the effect of their removal would be instantaneous, or gradual.

The following is an extract of the report of the committee:

"When the ship was in dock, on blocks perfectly straight, "she came down in the midships, by the sights placed on "the gun-deck, two feet two inches and a half; and by "those on the upper deck, two feet three inches and a quar-"ter; and when undocked, with the trusses complete, and "in their places, she hogged, or broke her sheer, by the "sights on the gun-deck, one foot two inches; and by those

" on the upper deck, one foot two inches and five eighths;
" and at the expiration of twenty-four hours she had hogged
" or further broke her sheer two inches and five-eighths,
" and then appeared stationary, and completely borne by the
" trusses.

" We then proceeded to take away the trusses in the
" hold, and when they were wholly disengaged, she further
" hogged, or broke her sheer, six inches. We next pro-
" ceeded to take away the trusses in the ports, and when
" they were wholly cleared, she dropped at the extremities,
" (or further hogged) three inches and a half, and was in
" the same position when tried twenty-four hours after.

" We further beg leave to state, that the whole of the
" trusses marked B, slackened as the ship floated from the
" blocks, and became short from half an inch to three inches
" and a half, and partook of no part of the pressure; which in
" our opinion clearly proves that the direction in which Mr.
" SEPPINGS has applied his diagonal frame is correct, as also
" the great utility of the trussing system; for although this
" ship, from her very defective state, was much against so
" severe an experiment, it has proved to us its good effects
" most satisfactorily; for many of the trusses in the ports
" forced *the timbers* three eighths of an inch within the ends
" of their covering planks, thereby lessening their effect from
" what it would have been if the ship had been of a sounder
" texture; yet on a ship in this state, the trussing between
" the ports alone, after those in the hold were wholly disen-
" gaged, had the effect of sustaining the immense pressure
" of both ends of the ship in her worst position, and pre-
" vented her from breaking, which she otherwise would

" have done, from three to four inches, and which she ac-
" tually and *immediately* did on their being disengaged."

This statement of the Portsmouth officers, I trust, will be considered conclusive as to the benefits to be derived from the principle of trussing in the construction of ships; and although it was only applied from the keelson to the beams in the hold, and not to the ribs or frame of the ship, as is the case when ships are regularly built on this system, yet it sufficiently establishes the soundness of the principle.

When the Justitia first floated, after being partially trussed as described, the noise occasioned by the pressure on the trusses is stated to have been " truly terrific," until she was fairly settled on them. The disengaging them also caused a similar crash.

As, previous to the abovementioned trial on the Justitia, some professional persons had expressed a doubt, whether the braces ought not to have been placed in the direction of the trusses, and *vice versa;* the following experiment was exhibited to show what was expected to take place, and which did actually take place on the trial made upon the ship.

Let No. 2. Plate I. represent a frame of wood, having the braces B pinned to the upper and lower ties C; let the trusses D and the longitudinal pieces E, be merely let in without any fastenings; then make the point F the fulcrum, and pressing down the ends GG, it will be found that the frame comes more in contact by the pressure. Next reverse the frame, and let H become the fulcrum, and by pressing at I, I, it will be seen that the trusses D and the longitudinal pieces E will immediately be disengaged and fall out; this proves that, had the long braces in the ships built on this

system in the diagonal frame, been placed in the same direction that those in this experiment were that are marked A, the ships so constructed would, in the act of launching, pitching in a sea, and as they grew old, have slackened certain parts of the diagonal frame, and the fabric would have been supported by long *crooked trusses*, whose ends would have had but one point of support, namely, the shelf piece or internal hoop; and what is more objectionable, the ends of the trusses would, if so placed, have been cut off to an angle of 45°. On the contrary, by making the ties in the diagonal frame the abutments, as many additional points of support are gained, as the trusses B exceed the braces A; and further, the trusses B are now straight and short, and their ends are cut off to a right angle, and thereby give a support, and the longitudinal pieces a fixedness, which would not have been the case had they been laid in the other direction.

Indeed no stronger proof could be adduced in favour of the efficacy of the principle, than that which was furnished in the launching of three ships of 120 guns (the Nelson, the St. Vincent, and the Howe). In form and dimensions these three ships are precisely the same, and their frames, beams, and external planking, of the same scantlings: the two former were built according to the old plan, and the latter upon the diagonal system. After the Nelson was launched, she was found to have altered nine inches and a half from her original sheer, and the St. Vincent nine inches and a quarter, while the Howe altered only three inches and five eighths. The whole machine in the case of the two former ships was generally disturbed; the Howe exhibited no such symptoms.

I shall only further state, that after the memorable battle

of Algiers, I requested the Navy Board to call upon Captain Coode, of His Majesty's ship the Albion, to report on the state of that ship, she being built on the new principle; and the following is an extract of his letter to them:

"I beg to inform you, that it is the opinion of myself and "the officers of the Albion, that it was impossible any ship "could have stood the concussion from firing, and the recoil "of the guns, better than she did; and on a very minute "inspection of the ship after the action, there was not the "least difference to be observed, except what had been made "by the enemy, between the side of the ship that all the "firing was from, and the side that not a single gun was "fired from during the action; and every bolt and knee was "as perfect and secure as before the action commenced, "which was also the case of the lower and main gun decks, "but the quarter deck was staved in several places; which in "my opinion would not have been the case, had it been on "the same construction as the decks that stood so well."

The Albion's decks, so well spoken of by Captain Coode, are laid diagonally; the quarter deck as usual, fore and aft.

To submit the diagonal decks to the test of experiment, I caused the decks of the Northumberland, of 80 guns, to be laid on one side fore and aft, as is usual, and on the other side diagonally, conformably with my principle; the materials on each side were of the same description, and the beams attached to both sides of the ship by the same mode.

This ship was ordered to convey General Bonaparte to St. Helena, and Rear Admiral Sir George Cockburn was, by the Lords Commissioners of the Admiralty, directed to report on the comparative merits of the decks. The following is an

extract of a letter from Captain Ross of the Northumberland, to Sir GEORGE COCKBURN, which was transmitted to the Admiralty:

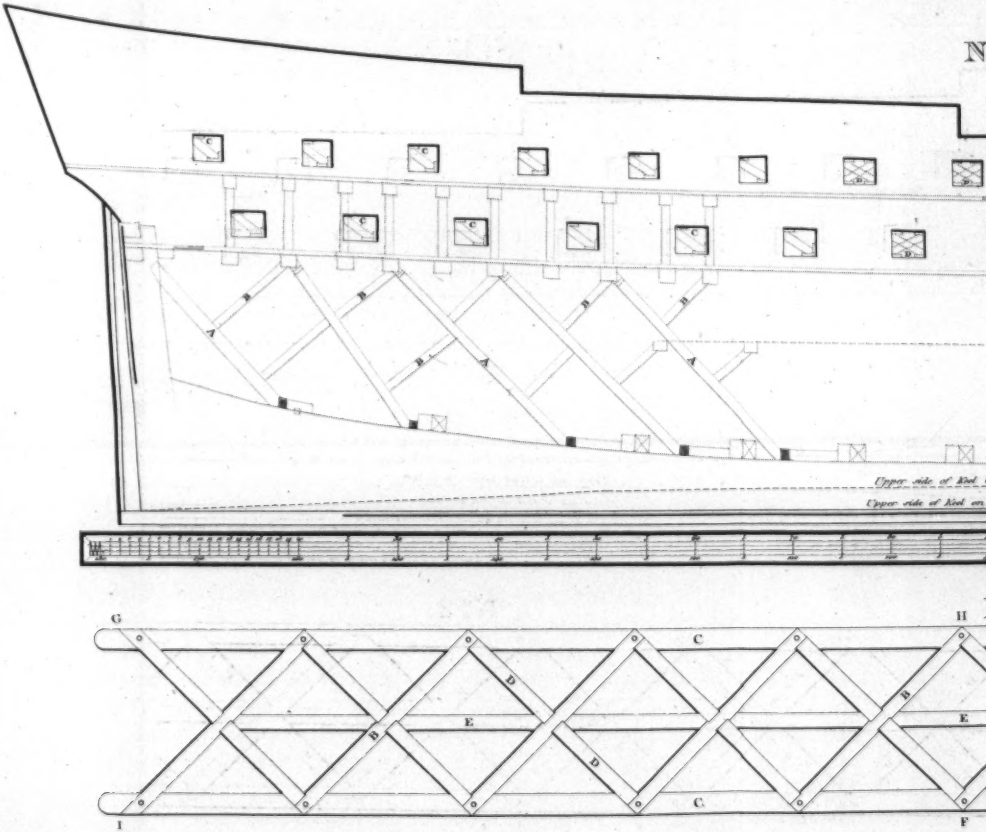
"I have to state, that the fore and aft side required caulk-"ing on the passage from England (which was partially "done) when the diagonal side did not; the fore and aft side "now requires caulking all over, and the diagonal side very "little; being, in my opinion, and that of the carpenter, "much in favour of the diagonal decks."

On the return of the Northumberland to Sheerness, the officers of the yard were directed very particularly to survey her decks. After speaking of the favourable report made to their enquiries by the officers of the ship, they stated as follows:

"This report of the officers was confirmed by the general "appearance of the ship on her arrival at this port, and "having subsequently caulked and minutely examined the "state of the decks and water-ways, we find the comparison "so much in favour of the larboard side, as to determine, that "the diagonal system of laying decks is preferable to the "common system."

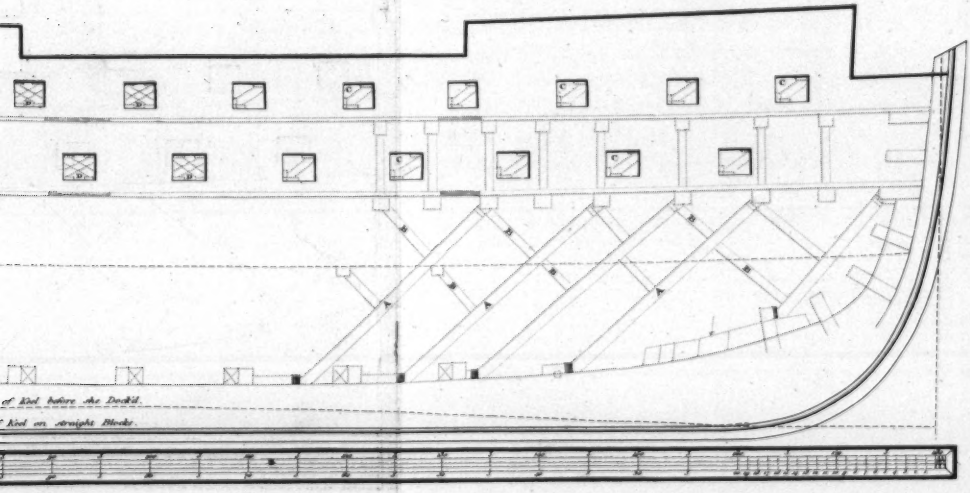
A Sketch, shewing the manner of applying th

His Majesty's Ship Justitia, of 74 Guns, prepar



g the Truſſes, in the Ports, and in the Hold, of
eparatory to a trial for ascertaining their effects.

N°. 1.



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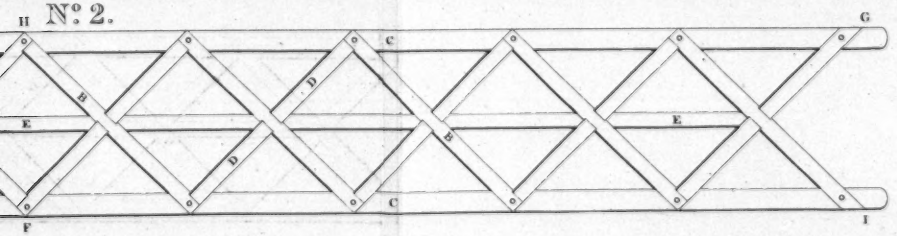


II. *A memoir on the geography of the north-eastern part of Asia, and on the question whether Asia and America are contiguous, or are separated by the sea.* By *Captain* James Burney, *F. R. S.*

Read December 11, 1817.

A BELIEF has prevailed for nearly a century, that the separation of America and Asia has been demonstrated by an actual navigation performed; and it is distinctly so admitted in the charts. It is proposed to show in this memoir, in the first place, that there does not exist satisfactory proof of such a separation; and secondly, that from peculiarities which have been observed, there is cause to suppose the fact to be otherwise; that is to say, that Asia and America are contiguous, and parts of one and the same continent. This is not an opinion newly formed, but one which many years ago was impressed on other persons as well as on myself, by circumstances witnessed when in the sea to the north of Bering's Strait with Captain COOK, in his last voyage.

America, from its first discovery by the people of Europe, was regarded by them as a land wholly distinct from their own native continent, till the failure of many attempts to discover a northern passage to India at length suggested the possibility that the Old and New World (as they were then called) formed but one continent. The solution of this problem, so far as regards a north-eastern navigation to India, has been more naturally the business of the Russians

than of any other people, as well on account of the greater facilities possessed by them for prosecuting the discovery, as for the superior benefit they would derive from a practicable navigation round their coasts to the Tartarian and Indian sea, should such be found.

The memorable voyage of SEMOEN DESCHNEW and his companions in 1648, by which the Russians first discovered the sea east of Kamschatka, (for before that time the river Anadir was supposed to run into the Icy sea), is the principal circumstance which has been admitted as proof of a complete separation of Asia and America. It is important to remark, that this admission is not so old as the expedition on which it is founded, by nearly a century; for no certainty of an absolute navigation having been performed round a north-eastern promontory and extremity of Asia was pretended till after the year 1736, when it was inferred by Professor MULLER, from some original writings found at that time in Siberia, concerning DESCHNEW's voyage. Baron de STRAHLENBERG, who had lived many years in Siberia, and whose description of that country is of earlier date than MULLER's publication, says of the expedition of 1648, that some Russians departed from the river Lena in boats towards the east, and by that route discovered Kamtschatka. But it was not understood to have been by a clear navigation round the N.E. of Asia; for in a description subsequently written, he says, "a class of people, to whom has been given the denomination "of Tartars, inhabit the north-eastern extremity of Asia, "concerning which a Kossak officer, named Atlassow, re-"ported, that between the Kolyma and the Anadir were two "great promontories, which he affirmed could not both be

" doubled by any vessel, because the west coast of the first " is barred in the summer by floating ice, and in winter the " sea there is frozen: but at the second, the sea is clear, " without ice."

Scheuchzer, the translator of Kæmpfer's History of Japan, in an introduction to his translation, cites some remarks which had been published concerning the Tartars, wherein it was said, " the inhabitants of Siberia who live near " the river Lena, and along the coast of the Icy ocean, in " their commerce with Kamtschatka, commonly go with " their ships round a Suetoi Noss [or sacred cape], to avoid " the Tschelatzki and Tschuktzki, two fierce and barbarous " nations possessed of the north-east point of Siberia." On this vague authority Scheuchzer concludes, that Asia is not contiguous to America.

When Mr. Muller first went into Siberia, no credited tradition appears to have been there current of the north-east extremity of Asia having been sailed round. Charts which were made in Siberia by people inhabiting the coasts of the Icy sea, showed *uncertainty*, and what is to be considered only as an expression of a *belief* of a great north-eastern promontory; for at that part, the coast was not defined by any outline, but left without limitation: whereas a more southern promontory, supposed the second from the Kolyma, was clearly delineated in the charts without any indication of doubt; and this last-mentioned promontory, it is evident, was the cape which was afterwards seen by Bering, and to which Captain Cook gave the name of Cape East, on account of its being the most eastern land known of Asia. In the instructions which were given by the Czar Peter the Great

for Captain BERING's voyage, the question whether Asia and America were contiguous or separate, was regarded as *undetermined*, and some Tschuktzki people, with whom BERING had communication, informed him that, " their countrymen " who traded with the Russians on the river Kolyma, always " went thither by land with their merchandize on sledges, " drawn by rein-deer, and that they had never made the " voyage by sea."

Mr. MULLER has acknowledged that from the perusal of the papers found concerning the voyage of DESCHNEW, he adopted a belief which did not before prevail, and he regarded it as a second discovery. Yet Mr. MULLER's own account fell very short of warranting a *certainty* of the manner in which DESCHNEW arrived at the Eastern Sea; and there is an irregularity in it which is perplexing. He says, ' DESCHNEW in relating his adventures speaks only incidentally of what happened to him by sea. We find no event ' mentioned till he had reached the great cape of the ' Tschuktzki. His relation,' says Mr. MULLER,' begins at this ' cape. It lies between the north and north-east, and turns ' circular towards the river Anadir. Opposite to the cape ' are two islands, on which were seen men through whose ' lips were run pieces of the teeth of the sea horse. With a ' favourable wind one might sail from here to the Anadir in ' three days and three nights.'

The cape or promontory which is here described is evidently the Cape East in Bering's Strait; and in a subsequent part of the account, DESCHNEW is represented to have said that this Noss ' on which the vessel of ANKUDINOW, (one of his companions) was wrecked, was not the first promontory

that had occurred, to which they had given the name of Swiætoi Noss.' The word Swiætoi signifies sacred, and is a name suitable to a promontory which could not be doubled. And this corresponds with the Siberian charts before noticed.*

It is necessary here to explain by what means the navigators in the Icy sea were enabled to arrive with their vessels at a second promontory, without having sailed round the first. On account of the frequency of being inclosed in the Icy sea, by the drift ice, it was customary to construct vessels in a manner that admitted of their being with ease taken to pieces; by which they could be carried across the ice to the outer edge, and there be put together again. The planks were fastened and kept to the timbers only by leathern straps, in lieu of nails or pegs. The construction of the vessels in which DESCHNEW and his companions went is not specified. Mr. MULLER calls them Kotsches. Baron STRAHLENBERG says they departed eastward from the river Lena in their *boats*.

In the beginning of the 18th century, the Czar PETER the Great sent directions to the Governor of Iakutzk to collect information concerning the discoveries which had been made. In consequence of this order, several examinations and depositions were taken; and the few authentic particulars which are known of the voyage of DESCHNEW were thereby preserved. The most remarkable of the depositions which are cited by Mr. MULLER, next to what relates to the expedition of

* It may be objected to this inference, that another cape in the Icy sea, although it has been sailed round, bears nevertheless the name of Swiætoi Noss; but it may naturally be imagined that the name was given before the difficulty had been surmounted.

DESCHNEW, is one which was made by a person named NIKIPHOR MALGIN, who stated that "a merchant named "TARAS STADUCHIN, did many years before relate to him, the "deponent, that he had sailed with ninety men in a Kotsche "from the river Kolyma towards the great cape of the "Tschuktzki: that not being able to double it, they had crossed "over on foot to the other side, where they built other vessels. "The small breadth of the isthmus at the part where they "crossed, is noticed as the most remarkable circumstance in "this deposition." They afterwards proceeded along the coast round the Kamtschatka Peninsula, till they came to the Penschinska gulf; and, in the short account which is given of this navigation, is found, expressed in an obscure manner, the *first* notice obtained by the Russians of the Kurilski islands.

This is a clearly described passage. Besides the expedition of DESCHNEW, and this of TARAS STADUCHIN, only one other instance is mentioned of any vessel having gone by sea from the Kolyma round the Tschuktzki coast; and this last mentioned case rests on the authority of an unauthenticated tradition, purporting that some man had gone in a vessel not larger than a skiff, from the Kolyma to Kamtschatka; and no other particular is spoken of in the report.

This was the state of the information obtained concerning the north-eastern extremity of Asia, at the time of Captain BERING'S voyage. The Asiatic side only of Bering's Strait was discovered in that voyage, and the coast of Asia being there found to take a western direction, it had the effect of giving an impression, equal to demonstration, of a total separation of Asia and America. And after that time, and not

before, Deschnew was believed to have performed the whole of his voyage from the Kolyma to the Anadir by sea.

Many reports had circulated in Siberia of the existence of northern lands in the Icy sea; but persons sent purposely to examine, had not found land, which much discredited the reports. A chart in which a northern land was marked was however published at Petersburgh, about the year 1626, by a Colonel Schestakow, of the Jakutzk Kossaks, a man of great ability as well as enterprise. Neither Schestakow nor his chart, however, are favourably noticed by Mr. Muller, who was in general a candid historian. On Schestakow's chart, the north land was marked with the name of the Large Country. M. de Lisle gave credit to Schestakow's map for the Large Country, which he makes appear on his own chart as a part of America, extending westward beyond the Kolyma.

Between the years 1734 and 1739, three expeditions were undertaken to ascertain the limits of Asia to the north and north-east, from which no advantage was reaped, and they were attended with circumstances of extraordinary distress and misery. These undertakings show that the boundary of Asia was not then regarded as ascertained. In 1764, a chart was sent from Siberia to Petersburgh, which again showed a continuation of the American continent stretching far to the west, and opposite to the Siberian coast of the Icy sea.

Between the years 1760 and 1765, no less than four attempts were made by one and the same individual, a Russian merchant, named Shalaurof, to sail from the Icy sea round the north-east of Asia. In the last of these attempts this en-

terprising and persevering man perished, for neither himself nor any of his people ever returned.

The information which was obtained in the first three attempts of SHALAUROF, is simply, that he arrived at an island which he named Sabedei, and beyond it sailed into a bay of the continent, which he named Tschaoon bay, which was estimated to be distant about 70 leagues to the east from the entrance of the river Kolyma. Here were found habitations and people.

Tschaoon bay ran deep into the land southward and eastward, and probably it was from this place that TARAS STADUCHIN crossed over to the eastern sea. Northward from Tschaoon bay, the coast took something of a westerly direction. The most advanced part of the land seen, was a high mountain far off to the north-east, SHALAUROF being then to the north of the island Sabedei.

Among the attempts to determine the north-eastern limits of Asia, is to be reckoned the march of a small Kossak army under the command of a Captain PAULUTZKI, which after traversing the Tschuktzki country, from the gulf of Anadir to the Icy sea, marched along the shore eastward, with intention to trace round the north-east coast; but the land being found to run far north, and their provision being expended, PAULUTZKI was obliged to relinquish the attempt.

Such was the state of the information which had been obtained, when Captain COOK arrived in the sea of Kamtschatka. Of three passages said to have been accomplished from the Icy sea to the Eastern sea, the manner of performing the voyage is distinctly expressed only in one; and that is speci-

fied to have been by crossing an isthmus, and not by sailing round a promontory.

I come now to speak of what was observed in the voyage of Captain Cook. The first extraordinary circumstance noticed on arriving in Bering's Strait, was a sudden disappearance of the tides. To the south of Bering's Strait, both on the Asiatic and on the American side, we had experienced strong tides. Near one of the Aleutian islands, where the ships had anchored, a tide was found running at the rate of seven miles per hour (as measured by the log) smooth and unruffled; at the same time, in the middle of the channel between this island and the next, the rapidity of the stream kept the waters in a foam during four hours of the tide.

Bering's Strait is formed at the narrowest part by two points, one named Cape Prince of Wales, which is the westernmost land known of America; and the other named Cape East, being the most eastern known land of Asia. Whilst we were to the south, and within sight of the Cape Prince of Wales, the wind and current, being in contrary directions, raised a sea that frequently broke over the ships. On arriving within Cape Prince of Wales, the ships anchored, the east cape of Asia then bearing due west; and it is remarked by Captain Cook, that whilst the ships lay there at anchor, which was from six to nine in the evening, there was found little or no current; nor could it be perceived that the water either rose or fell. Afterwards, whilst to the northward of Bering's Strait, we always had soundings of moderate depth, which enabled us to measure the stream with great exactness; and we seldom found one running at

the rate of more than half a mile per hour: at no time at the rate of a mile.

It is doubtless possible, that large bodies of ice taking the ground may choak up a channel between two seas, so as wholly to obstruct the tides; but it is not probable, that such should have been the case between this sea and the Icy sea, through the whole month of August and the beginning of September, to which time Captain COOK remained in the sea north of Bering's Strait. And the same stillness of the waters was observed there in the ensuing summer. The bottom also, not being swept by streams, was of soft ooze, so tenacious that the sounding line in common use was not strong enough to disengage the lead, and it became necessary to sound with a smaller lead and stronger line.

From Bering's Strait, Captain COOK coasted the land of America, to the north and north-eastward, as near as weather and other circumstances would admit, till, in latitude 70° 40′ N. his farther advance was stopped by a close body of ice to the N. and N. E. The ice, though compact, was not fixed, and was found to be approaching the American coast. Captain COOK remarks, 'as the ice was driving down upon us, it was 'evident, that if we remained longer between it and the land, 'it would drive us ashore, unless it should happen to take 'the ground before us.' Captain COOK on this, as on many other occasions, accommodated his views to the circumstances present, that there might be no unprofitable expenditure of time; and it may be said that in all his changes of plan, his measures were so directly adapted to his purpose, that without other communication his intentions were imme-

diately comprehended. The month of August was at this time far advanced; and to make the most of the short remainder of the season, Captain Cook stood on westward for the coast of Asia, keeping in as high a latitude as the ice would permit. On the north side of his track were extensive bodies of ice, such as we call field ice. These generally are accumulations of loose floating pieces, which have been brought together by the wind blowing a length of time in one direction towards a coast. When the ice is so driven to land, it is evident that the inner pieces only take the ground; the rest are confined by the wind, and when a change in the wind afterwards sets the ice from the land, it will preserve a position parallel to that which the coast gave it, until the strength and variety of winds have time to disperse it.

The deepest soundings we had in all this sea did not exceed thirty fathoms; and this depth was found in latitude 68° 45′, midway between the coast of Asia and the coast of America. Northward, beyond that latitude, the soundings were observed to decrease: and in our run from the coast of America westward, we did not find the depth to increase, as is usual in running from land. Which peculiarities made us conclude, that there was land at no great distance from us to the north, and that we were sailing on a line parallel with its coast. Northward of our track also, as we ran towards the Asiatic coast, was a continuity of ice which seemed as if formed into a close barrier by a long extent of coast.

The nature of the soundings, with the absence of tide, gave to this sea so much the character of a mediterranean sea, that some on board, in particular Mr. Bailey the astronomer, and myself, who being in the same ship communed

on the subject, were strongly of opinion that we were inclosed by land to the north, and that Asia and America were there joined; but we dared not venture to call in question the authority of MULLER.

If it is asked, whence then can come the great quantity of ice which is found in this sea? an answer readily presents itself. It is known that the Icy sea is frozen over every winter; and the northern part of this sea also has been known to be frozen over early in the winter. When the return of summer breaks up the ice, it will of course fill the sea with broken pieces.

Since the voyage of Captain COOK, little has been done towards ascertaining the termination of Asia. Commodore BILLINGS, an Englishman in the service of Russia, was employed to command an expedition for this express purpose, furnished with every assistance that could be devised towards ensuring success. His instructions directed him to make his departure from the river Kolyma, and to endeavour to follow the coast thence eastward by sea; with this additional instruction, 'that if coasting by sea should be found impracticable, 'and the information obtained on the spot should give hopes 'of effecting the purpose by land, he was then to endeavour 'to trace the coast by going in the winter in sledges over 'the ice.' Kossaks who had before been in the Tschuktzki country were selected to accompany Commodore BILLINGS, and among them was one who was the son of a Tschuktzki woman. 'Make agreement with them,' said the Instructions, 'or without agreement pay them the double of what is 'allowed to people who serve at sea. And whereas on a 'chart transmitted to us in the year 1764, a coast is marked

'opposite to the Kolyma, which stretches as a continuation
'of the continent of America, it will be of use if you can
'survey and describe the circumstances of that land.'

The history of Commodore BILLINGS's expedition may be told in few words. He sailed with two light vessels out of the Kolyma, on the 24th of June, 1787. He met with much ice, and on the 20th of July, without having reached so far eastward as the island Sabedei, he relinquished the farther prosecution of the attempt by sea; at the very season, in fact, which was the most proper that could have been chosen for his outset from the Kolyma.* In this short attempt, Commodore BILLINGS did not even get sight of the north land; but an approach to it was to be inferred from the soundings. The snow and ice were at this time rapidly dissolving, so as to cause currents to set for several days continuance in one direction; and during that time, the water on the surface of the sea was so fresh as to be used for cooking, and sometimes for drinking.

Afterwards, Commodore BILLINGS, with the consent of the Tschuktzki people, made a progress by land along a part of the Tschuktzki coast. Most unaccountably, he chose for his point of outset for this journey, the bay of Saint Lawrence, which is on the south side of Bering's Strait. He landed in the month of August with a party consisting of twelve persons, and travelled northward, keeping near the coast as far as to a bay called Klutchenie, which is at the extreme part of the Asiatic coast seen in Captain COOK's voyage. By this time, winter had set in, and the sea was frozen over. The

* His lieutenant (the present Admiral Saretcheff) proposed and offered to proceed north-eastward in a light boat; but his offer was not accepted.

season proved a severe one; the cold was extreme, and the whole party had already been so much fatigued and harassed with their journey from the bay of St. Lawrence, that they were unable to pursue the coast farther northward. They afterwards, in their route westward towards the Kolyma, crossed a river, which, according to information from the Tschuktzki people, discharged itself into the sea seventy versts more north than the bay of Klutchenie.

In all this uncertainty respecting the north-east termination of Asia, the particular most worthy notice is, that the Tschuktzki people themselves do not appear, from any of the accounts which have been published, to know the extent of their country to the north, or to be able to give any satisfactory information concerning it, though it is known that some of their nation have travelled from the continent to islands in the Icy sea. The charts of the present century, which have assumed to give a limitation to Asia, differ a degree in the latitude of their northernmost cape.

It does not in the smallest degree detract from the merit or fame of the first discoverers, to question their having navigated round the north-east of Asia. Whether they sailed round a promontory, or crossed an isthmus, they are equally entitled to the honour of having first discovered for their countrymen the sea east of Kamtschatka. The most probable chance of completing the discovery, or of arriving at any certainty concerning a north-eastern boundary of Asia, is doubtless that which was recommended by the Russian admiralty to Commodore BILLINGS; i. e. to trace the coast in sledges when the sea is frozen.

The principal argument, and it is not a weak one, against

the probability of Asia and America being joined, is, that northern land in the Icy sea has repeatedly been supposed, and reported, to be an extension of the American continent; and it does not appear in any of the accounts to have been reported, or supposed, to join the Tschuktzki country. In Captain KRUSENSTERN's memoir on the lands in the Icy sea, it is related, that very lately was explored an extent of 250 versts of coast of a northern land, which has been named the New Siberia. At the easternmost part which was seen of this land, the coast was observed to take a direction to the north-west. This direction of the coast might keep at a distance the supposition that it joined the Tschuktzki land: nevertheless, the coast may, and is supposed by the Russian discoverer, M. HEDERSTROOM, to turn afterwards to the east; for he gives it as his opinion that the New Siberia is a prolongation of America.

The Tschuktzki people would not explore farther north than afforded a prospect of reward for their pains. This, it is seen, has led them to some of the islands in the Icy sea; but no marks are noticed of their having been to the New Siberia.

The times for making expeditions of discovery in the Icy sea has generally been predetermined; but it would be more conducive to success to watch for favourable seasons. The state of the surface of the sea, when frozen, has also been found subject to much variation, depending upon the strength of the wind when the sea begins to be frozen. If in a calm, the surface will be smooth; if in boisterous weather, it will be rugged and bad for travelling.

III. *Additional facts respecting the fossil remains of an animal, on the subject of which two papers have been printed in the Philosophical Transactions, showing that the bones of the sternum resemble those of the ornithorhynchus paradoxus.* By *Sir* EVERARD HOME, *Bart. V. P. R. S.*

Read January 22, 1818.

MY first account of the fossil bones of this most extraordinary animal attracted the notice of geologists, and collectors of extraneous fossils, and led Mr. JOHNSTON of Bristol, and the Revd. Mr. BUCKLAND of Oxford, to assist me with the materials in their possession, to make a farther progress in the description of its skeleton. An account of these specimens formed the substance of my second paper.

Since that time I have frequently communicated with these gentlemen, also with the Rev. PETER HAWKER of Woodchester rectory, Minchinhampton, and Dr. CARPENTER of Lyme; and have received from them many specimens I had not seen before, some of which it was difficult to determine to what part of the skeleton they belonged: but that being ascertained, and a similarity discovered to bones of the ornithorhynchus paradoxus, that circumstance alone made them, in my opinion, of sufficient importance to become the subject of a third communication to this Society.

There is also another reason for bringing forward these facts, and for doing so without any unnecessary delay; for, as my former papers were the means by which I acquired them, their being made known to the public, may lead those

gentlemen who have opportunities of examining the cliffs in which the bones are found, to renew their labours, and assist in making out all the essential parts of the skeleton.

In the description I am to give of the bones received, I shall begin with one, a part of which is shown lying on the scapula in Mr. BULLOCK's specimen, engraved in the first Paper; it was then taken for the portion of a rib accidentally brought there, but it is now found to have been nearly in its natural situation. It bears a resemblance to the clavicular bone in birds. In the annexed engraving (Pl. II. Fig. 1.) it is shown in its relative situation to the other bones.

The bones of which the sternum is composed, are the next to be taken notice of: this part of the skeleton was first pointed out by my friend Mr. BUCKLAND, who had visited every collection in which bones of this animal were known to be preserved; he met with several specimens in which two flat bones were united together, and their union covered by a bone not unlike the upper bone of the sternum in quadrupeds, which made him believe that all the three formed part of the sternum. These different specimens, at his request, were sent to London for my inspection, and Mr. BUCKLAND's suggestion proves to be right.

This discovery of the sternum destroys the analogy between this animal and the cartilaginous fishes, which, while the materials were more scanty, I had been led to suspect, in consequence of the bones of the pectoral fin of the squalus having a greater degree of correspondence to the pectoral fin or paddle of the fossil animal, than any other bones I have examined.

As this form of the sternum appeared at the time quite new, I was very anxious not to fall into an error, and was re-examining, with Mr. CLIFT, the different specimens, when it struck him that there was something similar to this mechanism in the sternum of the ornithorhynchus paradoxus: this remark led to a comparison of the bones, and they were found to have a general agreement that could not have been expected.

In the ornithorhynchus, the first bone of the sternum, at its upper end, has two lateral processes, which are connected with a similar process from each of the scapulæ; underneath the first bone are two flat bones united together, which union is covered by the first bone. On the outer edge of these flat bones, there is a broad process continued down from the scapula; in this process is the hollow to which the first bone or os humeri of the pectoral fin is articulated.

It will be seen that the difference between these bones of the sternum in the fossil skeleton and that of the ornithorhynchus, consists in the fossil skeleton having a clavicular bone, which is wanting in the other; and the ornithorhynchus having a long process from the scapula, in which is the cavity of the shoulder joint, wanting in the fossil skeleton. These slight differences are not readily perceived in looking at the parts, as will be seen on inspecting the annexed engravings, Pl. II. Fig. 1, and 2.

The mechanism which has been described, gives a very unusual surface for the muscles attached to the sternum, which move the first bone of the pectoral fin; and upon examining them in the ornithorhynchus, I find there is not only

the great pectoral muscle, going from the first bone of the sternum to the first bone of the pectoral fin, and a small one under it which may be called the small pectoral muscle; but two large muscles, which have their origin from the flat bones and go to the first bone of the pectoral fin, or os humeri, and are inserted just below its head, that part being unusually broad, to allow of sufficient surface for their attachment. These muscles must be considered as peculiar to those animals that have this particular form of sternum.

As these muscles probably belonged to the animal whose fossil bones are under consideration, as well as to the ornithorhynchus, I have given them a particular description.

Till the sternum was discovered, it could not be ascertained in what manner the animal breathed; and the ribs being attached to the middle part of the vertebræ, as in fishes, made me lean to the opinion that it breathed water; but I now find, on more attentive consideration, that there is a difference between the mode of attachment of the ribs in this animal and in fishes, which admits of their having the motion of elevation and depression required in breathing air. In fishes, the attachment is single; but here, as is shown in the engravings in a former paper, it is double; and the two are considerably apart, as in the bird. Till it was determined whether the animal breathed air or water, little attention was paid to the nostrils, further than to find their place on the skull; more particularly as in the only head I had seen, the bony scales of the opposite eye had been pushed through an aperture situated where I considered the opening of the nostril must have been: but I

believed that the natural appearance of the orifice had been entirely destroyed. I have, since that time, through the favour of Mr. DELABECHE, of Lime, received a very accurate drawing of a fossil skull of this animal, in his possession, in which the apertures of the nostrils are very distinct; and he informs me that in all the skulls he has seen, the orifices are of the same size, and similar to that engraved in my first paper; so that there now can be no doubt respecting the real shape and size of the aperture which corresponds to that in birds; and as in birds there is no bony septum narium, the bony plates of the opposite eye being so distinctly seen behind the aperture, is readily understood.

To find any analogy between the bones of animals now alive, and those of animals so long extinct, that they must be considered to belong to another order of beings, is matter of no small curiosity; but to have discovered an analogy between the peculiarities met with in the bones of the animals of New Holland, by which they are so remarkably distinguished from all others that now inhabit our globe, and those of bones in a fossil state, not only creates a considerable degree of surprise, but at the same time, by connecting the present animals with those that are extinct, adds one to the intermediate links of the chain of gradation which must prove the most interesting to the comparative anatomist and the geologist.

The animal whose fossil skeleton we have been considering, is ascertained by the form of the vertebræ, to swim like a fish; and from the shape of its chest must breathe air; its constant residence must also be in the water. The ornitho-

rhynchus paradoxus, from the form of its vertebræ, swims like the whale tribe, and breathes air, but does not, like them, reside constantly in the water, as no animals covered with hair do.

As neither fishes, nor animals of the whale tribe, have any thing in the form of the sternum, or of the muscles that are attached to it, that is similar to what is met with in the fossil animal and ornithorhynchus, these last animals, in that respect, make an approach to the bird, in which the sternum has still greater breadth, and the muscles greater strength.

In finding that this structure of sternum is wanting in the whale tribe, it leads to an enquiry into the peculiarities of the habits of life of the ornithorhynchus, and the animal to which the fossil bones belonged, that make such a structure necessary.

The ornithorhynchus has its fauces and stomach commonly filled with sand and gravel, which renders it probable that it feeds at the bottom of the lakes and rivers in which it is met with; and as it must come to the surface to breathe at short intervals, this apparatus will enable it to rise with an unusual degree of velocity; and would answer the same purpose in the animal whose sternum has a similar construction. Nothing of this kind is required in the whale tribe; they are rendered sufficiently buoyant by the oil they contain.

There is a fossil bone found in the same place, having so nearly the same colour and general structure as those of this animal, that it seems to belong to it. In its form it bears a resemblance to the first bone of the pectoral fin, only much larger in all respects, it therefore may be conjectured to be

the first bone of the posterior fin, should the animal have had one; but till bones like those of the pelvis are found, this cannot be determined. It is engraved in Plate III. that it may be generally known, and by that means its place in the skeleton will be sooner ascertained.

The bones of the pectoral fin of the fossil skeleton, bear no resemblance to those of the onithorhynchus, but have some faint likeness to those of the pectoral fin of the shark. The bones of the pectoral fin of the ornithorhynchus correspond with those of the same fin in the whale tribe, turtles, the bones of the wings in birds, and of the fore leg of the quadruped. It would appear from the variety that occurs among the different specimens which have come under my observation, that the fossil bones met with at Lyme, not only belonged to animals of different sizes, but of different species. I am led to this opinion from Mr. DELABECHE having sent me two sets of the bones of the pectoral fin which evidently belonged to the same animal, in which the small bones of the paddle are set on to the long bone in a manner somewhat different to what it is in the others.

EXPLANATION OF THE PLATES.

PLATE II.

Contains two figures; the first represents the different parts that compose the sternum, in the fossil animal, and their connection with the neighbouring bones; the second the corresponding bones in the ornithorhynchus paradoxus, to show their resemblance.

Fig. 1. The sternum, and bones connected with it, in the animal whose bones are found in a fossil state.

aa Two flat bones peculiar to the sternum of this animal and the ornithorhynchus.

b A flat bone behind which is concealed the union of the edges of the two flat bones just mentioned.

cc Two processes from this bone, by means of which it is united to the scapulæ.

dd The two scapulæ, which, united to the flat bones, form the glenoidal cavities of the shoulder joints.

eee A slender curved bone crossing the upper end of the sternum, and uniting the two scapulæ together.

ff The two first bones of the pectoral fin or paddle.

Fig. 2. The sternum and scapulæ in the ornithorhynchus paradoxus.

aa Two flat bones nearly similar to those in Fig. 1. united to the scapulæ, but making no part of the glenoidal cavity of the joint of the shoulder.

b The bone corresponding to that marked *b*, Fig. 1.

cc Two processes similar to those in Fig. 1.

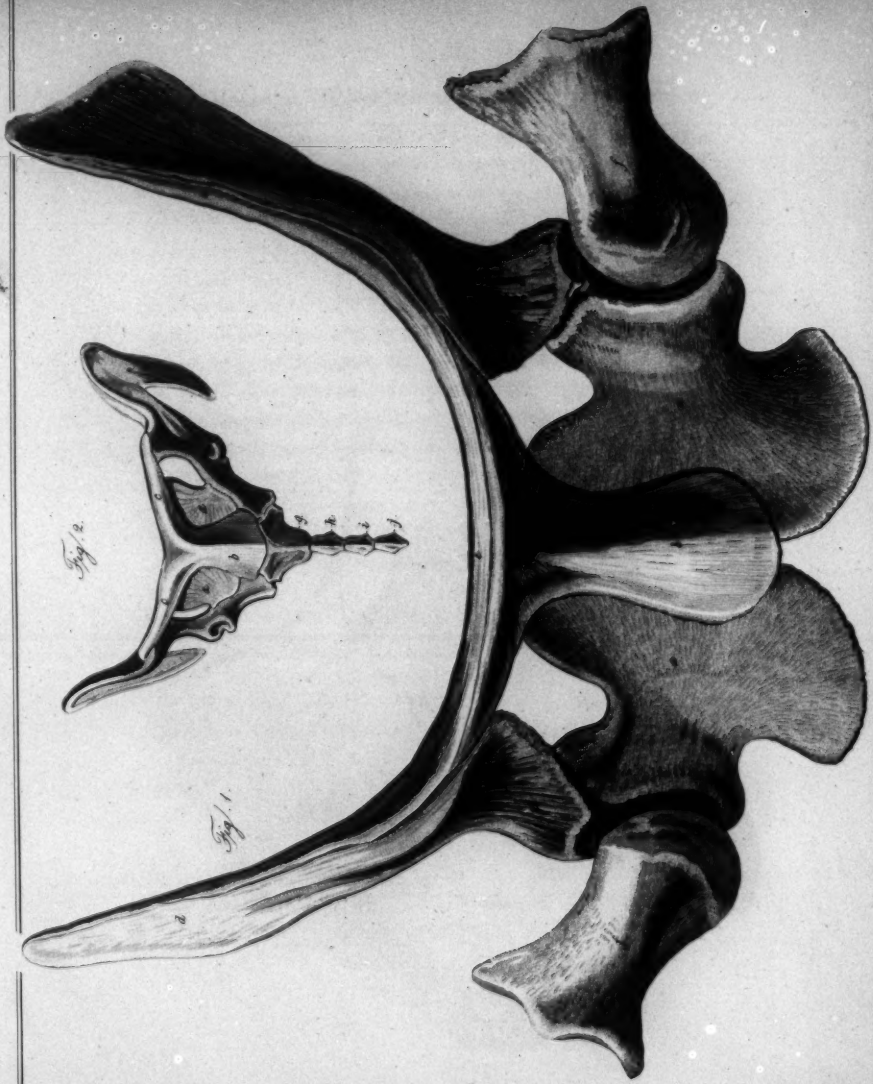
dd The two scapulæ which differ from those in Fig. 1. by extending below the two flat bones, and forming the whole of the glenoidal cavity of the shoulder.

g h i j Four bones belonging to the sternum.

All the parts are of the natural size.

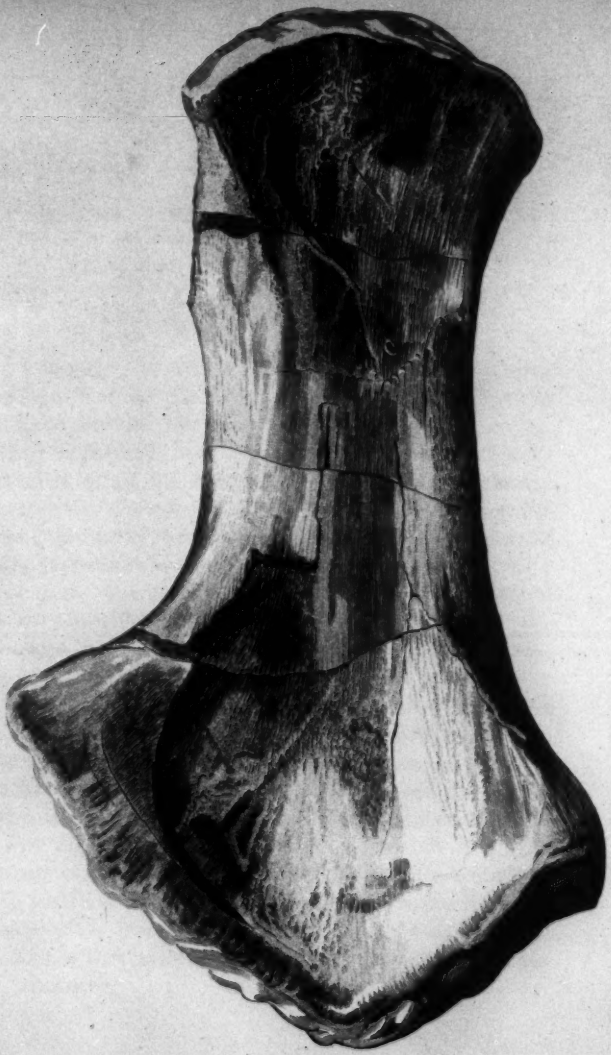
PLATE III.

The representation of a fossil bone of the natural size, which appears to belong to the same animal. Its place in the skeleton cannot at present be ascertained, since it is not yet known whether in this animal there is a regular pelvis. It bears a greater resemblance to the first bone of the paddle than to any other; so that if the animal has a posterior paddle, this must belong to it.



W. Clift del.

B. Basire sc.



W. Clift. del.

J. & C. Basire Sc.

IV. *An Account of experiments for determining the length of the Pendulum vibrating seconds in the latitude of London.* By *Capt.* Henry Kater, *F. R. S.*

Read January 29, 1818.

To determine the distance between the point of suspension and centre of oscillation of a pendulum vibrating seconds in a given latitude, has long been a desideratum in science. Many experiments have been made for this purpose, but the attention of all who have hitherto engaged in the enquiry (excepting WHITEHURST) appears to have been directed to the discovery of the centre of oscillation. The solution of this problem depending, however, on the uniform density and known figure of the body employed, (requisites difficult if not impossible to be ensured in practice,) it is not surprising that the experiments made by different persons should have been productive of various results.

When I had the honour of being appointed one of the committee of the Royal Society for the investigation of this interesting subject, I imagined that the least objectionable mode of proceeding would be to employ a rod drawn as a wire, in which, supposing it to be of equal density and diameter throughout, the centre of oscillation, as it is well known, would be very nearly at the distance of two-thirds of the length of the rod from the point of suspension; and I purposed by inverting the rod, and taking a mean of the results in each position, to obviate any error which might

arise from a want of uniformity in density or figure. After numerous trials however, and as frequent disappointments, I was at length convinced of the impracticability of obtaining a rod sufficiently uniform, and I was besides aware, that under certain circumstances errors might arise from this cause which it would be impossible by any method to detect.

Not feeling at all satisfied with the prospect which the use of a rod presented, I endeavoured to discover some property of the pendulum of which I might avail myself with greater probability of success; and I was so fortunate as to perceive one, which promised an unexceptionable result. It is known that the centres of suspension and oscillation are reciprocal; or in other words, that if a body be suspended by its centre of oscillation, its former point of suspension becomes the centre of oscillation, and the vibrations in both positions will be performed in equal times. Now the distance of the centre of oscillation from the point of suspension, depending on the *figure* of the body employed, if the arrangement of its particles be changed, the place of the centre of oscillation will also suffer a change. Suppose then a body to be furnished with a point of suspension, and another point on which it may vibrate, to be fixed as nearly as can be estimated in the centre of oscillation, and in a line with the point of suspension and centre of gravity. If the vibrations in each position should not be equal in equal times, they may readily be made so, by shifting a moveable weight, with which the body is to be furnished, in a line between the centres of suspension and oscillation; when the distance between the two points about which the vibrations were performed being measured, the

length of a simple pendulum, and the time of its vibration will at once be known, uninfluenced by any irregularity of density or figure.*

An unexceptionable principle being thus adopted for the construction of the pendulum, it became of considerable importance to select a mode of suspension equally free from objection. Diamond points, spheres, and the knife edge, were each considered; but as it was found difficult to procure diamond points sufficiently well executed, the knife edge was preferred, after many experiments had been made with

* In the Connoissance des Temps for 1820, is an article by M. de PRONY on a new method of regulating clocks. At the conclusion of this article is a short note, in which the author adds, "J'ai proposé en 1790 à l'Academie des Sciences un moyen "de déterminer la longueur du pendule en faisant osciller un pendule composé sur "*deux* ou trois axes attachés à ce corps. (voyez mes Leçons de Mécanique, art. "1107 et suivans) Il paroit qu'on a fait ou qu'on va faire usage de *ce moyen* en "Angleterre." On referring to the Leçons de Mécanique, as directed, I can perceive no hint whatever of the possibility of determining the length of the seconds pendulum by means of a compound pendulum vibrating on *two* axes, but it appears that the method of M. de PRONY consists in employing a compound pendulum having *three* fixed axes of suspension, the distances between which, and the time of vibration upon each, being known, the length of three simple equivalent pendulums may thence be calculated by means of formulæ given for that purpose. M. de PRONY indeed proposes employing the theorem of HUYGENS, of which I have availed myself, of the reciprocity of the axis of suspension and that of oscillation, as *one* amongst other means of simplifying his formulæ, and says, "J'ai indiqué les moyens "de concilier avec la condition à laquelle se rapportent *ces formules*, celle de rendre "l'axe moyen le reciproque de l'un des axes extrêmes; J'emploie pour les ajustemens "qu'exigent *ces diverses conditions* un poids curseur dont j'ai exposé les propriétés "dans un mémoire publié avec la Connoissance des Temps de 1817." Now it appears evident from this passage, that M. de PRONY viewed the theorem of HUYGENS *solely* with reference to the simplification of his formulæ; for had he perceived that he might thence have obtained *at once* the length of the pendulum without further calculation, the inevitable conclusion must instantly have followed that his third axis and his formulæ were wholly unnecessary.

spheres, the result of which it may not be useless for a moment to dwell upon.

It is known, that if two curved surfaces be ground together in every possible direction, they will become portions of spheres; and thus a perfect sphere may be formed by grinding a ball in a hemispherical cup. If a pendulum vibrate on such a sphere, working in a conical aperture, it is evident that the centre of the sphere will be accurately in the axis of vibration. In trying this method, however, it was found, that the friction was so considerable, as to bring the pendulum to a state of rest after a few vibrations; and when the friction was sufficiently diminished, by a contrivance which it is unnecessary to describe, the *lateral* force of the pendulum in an arc of two degrees and a half, was sufficiently powerful to carry the ball entirely out of the socket; and it was consequently evident, that though the arc of vibration might not be large enough to effect this, it must necessarily cause the ball in some degree to ascend the inclined plane of the aperture; and this consideration induced me to abandon at once a mode of suspension which I should otherwise have esteemed the best that could have been employed.

The principal objections to the use of a knife edge, appeared to be, the difficulty of forming it perfectly straight, and the possibility that it might suffer a change of figure from wear, during the experiments, which might introduce an error not to be detected. The first of these objections I found to be perfectly groundless, as a knife edge can be made so as not to deviate sensibly from a right line. The second objection would indeed be of weight, were the usual method of determining the time of vibration resorted to, by com-

paring the pendulum with a clock, at the distant intervals of 24 hours; but it will hereafter appear, that should any alteration in the form of the knife edge take place, it must become perceptible every ninth minute; in addition to which, I proposed to measure the distance of the knife edges both before and after the experiments, when any change would of course be immediately detected.

Description of the pendulum employed.

The pendulum constructed upon these principles is formed of a bar of plate brass, one inch and a half wide, and one eighth of an inch thick. Through this bar, two triangular holes are made, at the distance of 39,4 inches from each other, to admit the knife edges. Four strong knees of hammered brass of the same width as the bar, six inches long, and three quarters of an inch thick, are firmly screwed by pairs to each end of the bar, in such a manner, that when the knife edges are passed through the triangular apertures, their backs may bear steadily against the perfectly plane surfaces of the brass knees, which are formed as nearly as possible at right angles to the bar. The bar is cut of such a length, that its ends may be short of the extremities of the knee pieces about two inches.

Two slips of deal 17 inches long, and of the same thickness as the bar, are inserted in the spaces thus left between the knee pieces, and are firmly secured there by pins and screws. These slips of deal are only half the width of the bar; they are stained black, and in the extremity of each, a small whalebone point is inserted for the purpose of indicating the extent of the arc of vibration.

A cylindrical weight of brass, three inches and a half diameter, one inch and a quarter thick, and weighing about two pounds seven ounces, has a rectangular opening in the direction of its diameter, to admit the knee pieces of one end of the pendulum. This weight being passed on the pendulum, is so thoroughly secured there by means of a conical pin fitting an opening made through the weight and knee pieces, as to render any change of position impossible. A second weight of about seven ounces and a half, is made to slide on the bar near the knife edge at the opposite end; and this weight may be fixed at any distance on the bar by two screws with which it is furnished.

A third weight, or rather slider of only four ounces, is moveable along the bar, and is capable of nice adjustment by means of a screw fixed to a clamp, which clamp is included in the weight. This slider is intended to move near the centre of the bar. It has an opening, through which may be seen divisions, each equal to one twentieth of an inch, engraved on the bar; and a line is drawn on the edge of the opening to serve as an index to determine the distance of the slider from the middle of the bar.

We now come to the most important part, the knife edges. These are made of that kind of steel which is prepared in India, and known by the name of wootz. Their form is triangular, and their length one inch and three quarters. Mr. STODARD was so obliging as to forge them for me: they were made as hard as possible, and tempered by immersing them merely in boiling water.

The knife edges were ground on a plane tool, which necessarily ensured a perfectly straight edge. This was ascer-

tained by bringing the edge of the one in contact with the plane of the other, when, if no light was perceptible between them in any position, it was inferred that the edge was a right line. They were then carefully finished on a plane green hone, giving them such an inclination as to make the angle on which the vibrations are performed about 120 degrees.

Previously to the knife edges being hardened, each was tapped half way through, near the extremities, to receive two screws, which being passed through the knee pieces, drew the knife edges into close contact with them, the surfaces of both having been previously ground together to guard against any strain which might injure their figure. A plan of the pendulum is given in Plate IV. Fig. 1.

The support, and other apparatus.

The support of the pendulum is represented in Plate IV, Fig. 2. It consists of a piece of bell metal six inches long, three inches wide, and three eighths of an inch thick. An opening is made longitudinally through half the length of the piece, to admit the pendulum, and the bell metal is cast with a rectangular elevation on each side of the opening extending the whole length of the piece. Two plates of agate* were cemented to this elevated part, beds having been made to receive them, in order that their surfaces might be in the same plane with that of the bell metal. The whole was then ground perfectly flat. A frame of brass represented at Fig. 3, is attached by two opposite screws, which

* Plates of hard steel were first tried, but were found to have suffered penetration by the knife edge.

serve as centres, to the sides of the elevated part of the support, and one end of this frame being raised or depressed by means of the screw A, the pendulum when placed with its knife edge resting in Ys, at the other end of the frame, could be elevated entirely above the surface of the agate, or be gently lowered until the knife edge rested wholly upon it, and thus the knife edge was sure to bear always precisely on the same part of the agate plane, by elevating the Ys, above its surface, placing the knife edge in them, and then letting down the whole gently by means of the screw, till the Ys were completely clear of the knife edge. The support was firmly screwed to a plank which will hereafter be described.

To the kindness of HENRY BROWNE, Esq. F.R.S., I am essentially indebted for the success of the experiments which form the subject of this paper. He most obligingly allowed me the use of his house, his excellent time-pieces, and transit instrument, assisting me with indefatigable zeal by his very accurate daily observations, and intermediate comparisons for determining the rate of the clock. The house is substantially built, and is situated in a part of Portland Place not liable to much disturbance from the passing of carriages. The room in which the experiments were made is the last of two on the ground floor, communicating with each other and facing the north. The temperature consequently is very steady, and if necessary, may be raised to any given degree by a fire in the first room. The clock with which the pendulum was compared was made by ARNOLD; and in addition to the gridiron compensation for temperature, its pendulum is suspended by a spring, the strength of which is so adjusted, that the vibrations in different arcs are performed in equal times. This

clock is firmly screwed to the wall, in a recess opposite to the window. Near to this, on the wall which is at right angles to the recess, is fixed another time-piece by CUMMING, which was the property of the late General ROY, and is considered by Mr. BROWNE to be the best in his possession. Respecting this clock, it will be sufficient to remark, that three tenths of a second was the greatest variation in its daily rate from the 22d February, when the observations commenced, to the 31st July; and consequently the deviation from its mean rate during that period, did not exceed 0,15 of a second per day. This clock has been used as the standard of comparison, the time having been taken from the transit instrument by a chronometer of ARNOLD's. With such advantages it will be confessed that there can be little chance of error arising from the rate of the clock.

A plank of well seasoned mahogany, two feet wide, and three inches thick, was forcibly driven between the walls forming the sides of the recess, until it was near the top of the clock case. To this the support of the pendulum before described was firmly screwed, and carefully levelled, in such a position as to allow the pendulum to vibrate as near as possible to the clock case without touching it; and that when at rest, it might appear to an observer in front of the clock, to pass over the centre of the dial plate, its extremity reaching a little below the centre of the ball of the pendulum. Beneath, fixed to the clock case, was an arc divided into degrees and tenths, to determine the extent of the vibrations. Such a portion of the plank was cut away as was necessary to admit of the pendulum being placed on its support. A circular white disk was pasted on a piece of black paper,

which was attached to the ball of the pendulum of the clock; and this disk was of such a diameter as when both pendulums were at rest, to be just hid from an observer at the opposite side of the room, by one of the slips of deal which form the extremities of the brass pendulum.

Though there was little reason to imagine that the vibrations of the pendulum could communicate any motion to a support so firm as that which has been described, it became a point of considerable importance to verify this by actual experiment. For this purpose I had recourse to a delicate and simple instrument invented by Mr. HARDY, clock-maker, the sensibility of which is such, that had the slightest motion taken place in the support, it must have been instantly detected. This little instrument is represented in Plate IV. Fig. 4. It consists of a steel wire, the lower part of which inserted in the piece of brass which serves as its support, is flattened so as to form a delicate spring. On the wire, a small weight slides, by means of which it may be made to vibrate in the same time as the pendulum to which it is to be applied as a test. When thus adjusted, it is placed on the material to which the pendulum is attached; and should this not be perfectly firm, its motion will be communicated to the wire, which in a little time will accompany the pendulum in its vibrations. This ingenious contrivance appeared fully adequate to the purpose for which it was employed, and afforded a satisfactory proof of the stability of the point of suspension.

A firm triangular wooden stand, as high as the ball of the pendulum, was screwed to the floor at the distance of nine feet in front of the clock. This served as a support, to which was attached a small telescope, magnifying about four times,

which was capable of a horizontal motion on its axis, a vertical motion, and a motion at right angles to the line of sight. In the focus of the eye-glass was a diaphragm forming a perpendicular opening, the sides of which were parallel, and capable of being placed nearer, or further asunder. The edges of this diaphragm were adjusted so as to form tangents to the horizontal diameter of the white disk, and consequently to coincide with the edges of the slip of deal. When, therefore, both pendulums were at rest, nothing was visible through the telescope, excepting the divided arc for ascertaining the extent of the vibrations, and which was seen through a horizontal opening made for that purpose in the top of the diaphragm.

Method of determining the number of vibrations made by the pendulum in 24 hours.

If both pendulums be now set in motion, the brass pendulum a little preceding that of the clock, the following appearances may be remarked. The slip of deal will first pass through the field of view of the telescope at each vibration, and will be followed by the white disk. But the distance between the centres of suspension and oscillation in the brass pendulum being rather the longer, the pendulum of the clock will gain upon it, the white disk will gradually approach the slip of deal, and at length, at a certain vibration, will be wholly concealed by it. The minute and second at which this total disappearance is observed, must be noted. The pendulums will now be seen to separate, and after a time will again approach each other, when the same phenomenon will take place. The *interval* between the two coincidences in seconds, will give the number of vibrations made by the pendulum of the clock; and the number of oscillations of the

brass pendulum, in the same interval, may be known by considering that it must have made two oscillations less than the pendulum of the clock. Hence by simple proportion, as the vibrations made by the pendulum of the clock, are to the number of vibrations made by the brass pendulum, so are the vibrations made by the pendulum of the clock in 24 hours, to those of the brass pendulum in the same period.*

Many experiments were made in order to select such a distance of the knife edges as might give an interval which would allow of the determination of the time of coincidence without an error of a single second,† and yet afford a convenient number of intervals before it should become necessary to renew the motion of the pendulum. At the first coincidence, the velocity of the brass pendulum, at the lowest part of the arc, must not exceed that of the pendulum of the clock, otherwise the disk would disappear for an imperceptible time, and then re-appear; and this limits the extent of the arc of vibration.

Again; the observations must not be continued beyond a certain diminution of the arc of vibration, otherwise the *space*, which the pendulum of the clock has to gain on the brass pendulum in one vibration, becomes so small as to render the observation of the time of coincidence in some degree uncertain; and, should the *space* be so far diminished as to be less than the error or deviation from a right line, which would

* In order to render the calculation more easy, the clock has always been supposed to keep mean time, or to make 86400 vibrations in 24 hours, and the variation from this number, or the rate of the clock (being a very small quantity) has been afterwards applied as a correction.

† The *principle* on which this method of coincidences is founded, was employed by Dr. WOLLASTON, in May 1808, in some experiments in which he was then engaged, the moment of coincidence being determined however by sound instead of sight.

probably take place in the adjustment of the sides of the diaphragm, the end of the pendulum, and the disk, the results would be erroneous, as the interval would go on increasing till the pendulum came to a state of rest.

The interval which best fulfilled these conditions was found to be about 590 seconds. This admitted five coincidences (affording four intervals) to be taken before the arc became too small for the observations to be continued with safety. With this interval an error of one second in the time of coincidence would occasion an error of only 0,63 in the number of vibrations in 24 hours.

Here it must be evident that no sensible alteration could take place in the knife edge during the experiments without its becoming perceptible at every coincidence; since the number of vibrations in 24 hours deduced from each interval, must vary with any change in the form of the knife edge.

The following was the method pursued in making the observations. The small weight or slider being placed with its index at a certain distance (say one inch and a half) from the middle of the pendulum towards the great weight, and the second weight about five inches from the knife edge, the Ys of the support were elevated, the knife edge of the pendulum was placed in them, with the great weight *above*, and the frame gently lowered till the knife edge was left on the surface of the agate. The requisite adjustments of the telescope having been made, the pendulum was set in motion in an arc not exceeding one degree and four tenths, in order that its velocity might not be greater than that of the pendulum of the clock.

The minute and second, at which the disk ceased to be visible, was then carefully noted; and the arc of vibration seen through the telescope, the height of the barometer, and the temperature indicated by a thermometer suspended on the clock case near the middle of the brass pendulum, were also observed and registered. Five successive coincidences were thus taken, and the number of vibrations in 24 hours was deduced from them in the manner before described; but the vibrations thus obtained being made in different arcs, it became necessary to apply a correction to determine what they would have been in an arc infinitely small. For this correction I might have used a formula depending on the decrease of the arcs in geometrical progression, whilst the times decrease in arithmetical; but as there is an uncertainty in observing the arc of vibration amounting to one or two hundredths of a degree, this method, though more perfect in theory, would have been an unnecessary refinement in practice.

The error arising from the greater length of the vibration in a circular arc, being nearly as the square of the arc, if the mean of the observed arcs at the commencement and end of each interval be taken, and its square multiplied by 1,685 (the difference between the number of vibrations made by the pendulum in 24 hours, in a cycloid and in an arc of one degree,) the required correction will be obtained, to be added to the number of vibrations before computed.

The mean of these last results being taken, and also the mean of the observed temperatures at the first and last coincidences, the number of vibrations in 24 hours was obtained

at a certain temperature, and altitude of the barometer, in an infinitely small arc, the great weight being *above*.

The frame of the support was now elevated, the pendulum was inverted, placed in the Ys, with the great weight *below*, and the knife edges being gently let down as before on the agate plane, the same process with respect to the observations was followed, which has just been described. And if the mean temperature differed from that in the former position of the pendulum, the mean number of vibrations was corrected for such difference of temperature, the expansion of the pendulum being known by experiments hereafter to be detailed, and consequently the gain or loss in 24 hours by a given change of temperature.

The mean number of vibrations thus found, differing from that given in the former position of the pendulum, the second weight was moved, the number of vibrations again determined, and the pendulum being inverted, the process was repeated until the vibrations in 24 hours, in either position of the pendulum were brought as near to an equality as could readily be effected by means of this weight; it was then firmly secured in its place.

Whatever alteration may be made in the arrangement of the weights, the effect on the vibrations (except in one particular instance) will be the same in both positions of the pendulum, always increasing or diminishing their number in both cases, though in different degrees; and, the vibrations will be least affected by such change when the great weight is below, and will consequently be nearest to the truth in this position. No doubt, therefore, can arise, as to the kind of correction required. The number of

vibrations after the adjustment by the second weight has been completed, must be left *in defect*, for a reason which will be immediately apparent.

There is a point in the pendulum where the effect of the slider in increasing the number of vibrations is a maximum; and it appears from Dr. YOUNG's investigations, that this point in one position of the pendulum is different from that in the other. *Very near* either of these points, the pendulum being in its corresponding position, the motion of the slider produces scarcely any change in the number of vibrations; but the slider being then more distant from the point of maximum belonging to the other position of the pendulum, the corresponding increase of the number of vibrations arising from such motion of the slider, will, in that position be very perceptible.

In the present instance, the point of maximum in either position of the pendulum, is about four tenths of an inch below the middle, and consequently the distance of the two points from each other, is about eight tenths of an inch. The slider which had remained stationary during the adjustment of the second weight at about one inch and a half from the middle of the pendulum towards the great weight, must now be shifted, (say one inch) towards the middle of the pendulum, in order to increase the number of vibrations which it may be recollected were left in *defect*, so that they may be in *excess*. It is evident that the true number of vibrations will be found, when the slider is somewhere between its first and second position. Let the slider be now placed half-way between these two points. If the number of vibrations in this

third position be still in excess, the truth will lie between the first and third positions of the slider. And thus by continually bisecting with the slider, the distance of the two last found points, the number of vibrations when the great weight is *below*, will rapidly approach the truth, being alternately in defect and in excess; and when the approximation is such as that the difference in either position of the pendulum becomes inconsiderable, the vibrations, when the great weight is below, may be taken for the truth; and thus the number of vibrations in 24 hours, of a pendulum equal in length to the distance between the knife edges, will be known at a certain temperature, and at an observed height of the barometer. The general arrangement of the apparatus is represented by Plate V.

Of the apparatus and methods employed for the measurement of the distance between the knife edges, and for the comparison of the British standard measures of the highest authority.

The microscopes used for this purpose, were made by Mr. Thomas Jones, of Cockspur-street. They are both furnished with cross threads of spider's web, as well as with a single thread for the purpose of bisecting a dot if required, and are in other respects of a similar construction with those described by Sir George Shuckburgh Evelyn, in the Philosophical Transactions for 1798, but are more powerful, and the micrometer is capable of far greater precision.

The object glass of the micrometer microscope is of one inch focus, the distance from the object glass to the spider's threads 3,25 inches, the focus of the compound eye-glass rather less than one inch, the magnifying power 18 times.

In the other microscope, which I shall call the fixed microscope, the object glass is of three quarters of an inch focus, and the magnifying power consequently greater. The micrometer head is divided into one hundred parts.

Each microscope slides in a tube, which is fixed in a plate of brass forming part of its support; and this plate moves in a dovetail, by which the microscope may be brought over the object to be viewed, when it is firmly clamped by a screw.

A piece of well seasoned mahogany, four inches and three quarters, by three inches, served as a beam to which the supports of the microscopes were screwed, their centres being 39.4 inches asunder.

Two screws with milled heads, supported the extremities of the beam in front, and a piece projecting from the middle of the beam behind, served as a third leg. By means of the screws, the focus of either microscope could be nicely adjusted at pleasure, without any risk of altering their distance from each other.

My first object was to ascertain the degree of precision of which vision is capable when assisted by the microscope. For this purpose, a very fine line was drawn on a polished piece of brass, and the microscope being carefully adjusted so as to be free from parallax, by causing the image of the line to bisect the angles formed by the spider's threads, moving the eye to the right and left and remarking whether the image changed its situation, and if it did, varying the distance of the microscope from the object accordingly, until the line appeared stationary, the micrometer screw was turned back, and the spider's threads brought up again till the angle

formed by them appeared to be accurately bisected by the line. The division of the micrometer was then noted, and this was repeated several times with scarcely a sensible difference in the result; and thus I assured myself that no error worthy of remark was to be apprehended from imperfection of vision.

The next step was to determine the value of one division of the micrometer head. By the kind interest of Sir Joseph Banks, I was favoured with the use of the standard scale which belonged to the late Sir George Shuckburgh Evelyn, and which is described in the Philosophical Transactions for 1798. This scale, the work of Mr. Troughton, is second to none in the kingdom in point of accuracy of division, and is too well known to render any further remark necessary. The microscope being carefully adjusted for parallax, one inch, from the 39th to the 40th, was measured by successive tenths, and the mean taken as the value of one tenth of an inch. The measurement of the same inch was repeated ten times at different periods, the microscopes being previously adjusted anew each time for parallax. The mean results of such measurements are as follows.

Divisions of the micrometer to $\frac{1}{10}$ of an inch.

2335,00
2333,75
2337,55
2337,32
2334,50
2336,90
2335,75
2338,30
2335,85
2337,85

Mean 2336,277

Hence, the value of one division of the micrometer appears to be $\frac{1}{233.83}$ of an inch.

In the course of these measurements, differences occurred for which I was at a loss to account; but at length it appeared that they were to be attributed to remaining parallax; for whatever care be taken in adjusting the microscope, it is scarcely possible to bring the image of the object precisely in the same plane with the threads, and the image will consequently be of various dimensions, according to its distance from this plane. Unless, therefore, the most minute attention be paid to the adjustment for parallax, the error arising from this cause will be considerable; and I may here remark that I believe the difficulty of bringing the image into the plane of the threads, to be the source of by far the most serious errors to which measurements by means of microscopes are liable.

I had now to examine the equality of the threads of the micrometer screw. For this purpose, two fine lines were drawn near each other on a piece of brass, and the micrometer being turned back as far as it would go, the distance of the lines was carefully measured; and this was repeated, proceeding through the whole length of the screw, always advancing the micrometer one revolution previous to each successive measurement. The result of this severe test will best appear by giving the numbers themselves.

Divisions of the micrometer.

502,0
501,5
501,0
502,0
501,5
502,0
502,0
502,5
502,0
502,0
502,5
501,0
501,5
502,0
501,0
502,5
501,0
500,0
500,0
500,5
Mean 501,5

The mean is 501,5, and the greatest difference from the mean only one division, amounting to $\frac{1}{23563}$ of an inch, a degree of accuracy truly surprising, when it is considered that all errors of observation are included in this minute quantity.

Comparison of the different standards.

The microscopes being placed at the distance of 39,4 inches, were advanced by single tenths, from zero of the scale through the space of two inches, and the mean of twenty measurements thus obtained being compared with the distance from zero to 39,4 inches, this last was found to be

in defect 1,2 divisions of the micrometer, or ,00005 of an inch. And as this is the portion of the scale employed in ascertaining the distance between the knife edges, this difference must ultimately be subtracted to obtain the distance of the knife edges, in parts of the mean value of the scale.*

From the high importance which attaches to General ROY's scale, as having formed the basis of the Trigonometrical Survey of the kingdom, I was particularly desirous of comparing it with that of Sir G. SHUCKBURGH, in order that I might be enabled to give the length of the pendulum in parts of that standard which constitutes the foundation of one of the most important scientific operations ever carried on in this country. Fortunately, this scale was purchased at the sale of General ROY's effects by Mr. BROWNE, who readily confided it to my care. From the mean of a number of comparisons, I found the distance from zero to 39.4 of General ROY's scale, equal to 39,40144 of Sir G. SHUCKBURGH's standard.†

* From an examination of this scale by the late Sir G. SHUCKBURGH, it appears that the greatest liability to error is ,00033 of an inch, or as corrected by Mr. TROUGHTON, ,000165 of an inch, the chances against which are as 9 to 1.

† The very great difference between this result and that stated by Sir GEORGE SHUCKBURGH, in the Philosophical Transactions for 1798, renders it necessary for me briefly to detail the manner in which the comparisons were made. The two scales were placed in contact, and remained thus for twenty-four hours; after which, sixteen comparisons were taken in the course of the day; but these were rejected in consequence of the temperature having increased six degrees during the operation. When the scales had been together forty-eight hours, sixteen other comparisons were made during two succeeding days, the thermometer remaining steadily at 70°. The greatest difference between any one of these last and the mean result, did not amount to four divisions of the micrometer. The mean of the first set of observations ex-

The standard yard made by BIRD in 1758, for the House of Commons, better known by the name of BIRD's Parliamentary standard, is little adapted for measurements where great precision is necessary. The yard is determined by two large dots made on gold pins, which are let into a bar of brass. The mean of a number of bisections of these dots gave their distance equal to 36,00016 inches of Sir GEORGE SHUCKBURGH's scale.

Measurement of the pendulum.

The pendulum was let into a solid piece of mahogany edgewise, to such a depth that the knife edges were about one twentieth of an inch above its surface. To one end of the pendulum, a common spring steelyard was attached by its hook, and a string being passed through the ring, and fastened to an upright piece of wood screwed to the end of the mahogany case, the pendulum was extended by a force rather greater than its own weight (about ten pounds), and consequently, no error (if any such were to be apprehended)

ceeded that of the last by ,00017 of an inch. Imagining that the difference between Sir GEORGE SHUCKBURGH's result and mine, might possibly be occasioned by an error in the divisions bounding that part of General ROY's scale which I had employed, I compared it with various other portions, and found no greater difference than might have been expected from unavoidable imperfection of division. It is to be presumed then, that the error into which Sir GEORGE SHUCKBURGH appears to have fallen, must have arisen from the two scales not having been of the same temperature at the time they were compared, particularly as Sir GEORGE SHUCKBURGH's is by far the most massive of the two. I may here add, that last winter wishing to know whether the expansion of the two scales was equal, I roughly compared them together once, at the temperature of 33°, when it appeared that 42 inches on General ROY's scale, was equal to about 42,001 inches of Sir GEORGE SHUCKBURGH's standard.

could arise from a difference in the length of the pendulum in its vertical and horizontal positions.

The knife edges were fixed as nearly as could be done by mechanical means, at right angles to the bar of the pendulum; but the bar being flexible, they would most probably, when the pendulum was extended for the purpose of measurement, be found to be not precisely parallel to each other, and would consequently require some adjustment. To effect this, two opposite screws were passed through the sides of the mahogany case, so as to act in a transverse direction against that extremity of the pendulum which was next the steelyard, and the microscopes being brought over the extreme points of the knife edges, alternately on either side of the bar, the requisite parallelism was readily obtained by means of the screws, sufficient room having been left in the mahogany case for the very small motion of the extremity of the pendulum which might be found necessary. This arrangement is represented in Plate IV. Fig. 5.

To obtain the distance between the knife edges, two different methods were used. For the first, four rectangular pieces of brass were prepared, about half an inch square. Very near to the perfectly straight edge of each, a fine line was drawn, to be viewed through the microscope, and these lines were each crossed at right angles by two others, intended to indicate that part of the first line from which the measurements were to be taken. These pieces were marked A, *a*, and B, *b*.

The pieces A and *a*, being placed with their edges in contact, in which position they were kept by the pressure of a spring, the distance between the fine lines first drawn was

carefully measured with the micrometer, and from a mean of eight observations, the greatest difference between which did not exceed one division, was found to be 829,09 divisions.

The same was done with the pieces B and b, and the distance of the lines from a mean of sixteen observations appeared to be 866,96 divisions.

The knife edges being adjusted as nearly as possible parallel to each other, the pieces A, a, and B, b, were placed in contact with those parts of the knife edges on either side of the bar, on which the vibrations were to be performed, and were retained in their places by the pressure of slight springs, attached to the mahogany case.

The microscopes were now brought over the pieces A and a, so as for the lines before described to bisect the cross threads, when the division of the micrometer was noted.

The same was done with the pieces B and b; and the division of the micrometer was also registered.

The pendulum being removed, the standard scale* was placed beneath the micrometer, and its zero being made to bisect the angles of the fixed microscope, the cross threads of the micrometer microscope were brought to 39,4 of the scale, and the revolutions and parts of the micrometer were noted.

From these data, and the respective distances of the lines on A and a, and on B and b, when the pieces were in contact, the distance of the knife edges on either side of the bar may be readily obtained, and the mean being taken, will obviously correct any error arising from a want of perfect parallelism in the knife edges.

It is very generally believed that measurements from a

* The scale constantly referred to, is Sir GEORGE SHUCKBURGH's standard.

knife edge, or from a line terminating a surface, are liable to much uncertainty from what has been called *irradiation*, or indistinctness of the image. But this is by no means the fact; for if the reflection of light from the knife edge be prevented, and it be viewed on a white ground, it may be made to bisect the cross threads of the microscope, with nearly the same precision as could be attained by the use of a line. There is, however, a correction necessary to be applied in this case, and I shall proceed to describe the method employed for ascertaining its amount.

A slip of writing-paper was pasted on the mahogany case, under each knife edge, extending beyond it about the tenth of an inch, and adjoining, was a piece of black paper to prevent the reflection of light on the knife edge from the surrounding objects. The knife edge now appeared through the microscope, as a well defined dark object on a white ground.

Marks were made on the paper close to the knife edges at equal distances on each side of the bar. These were intended to indicate those parts of the knife edges equally distant from the middle, from which the measurements were to be taken.

The knife edges being adjusted parallel to each other, in the manner before described, the microscopes were brought successively over such marks on the paper, as were at the same distance from the bar, and the mean of each pair of observations being referred to the scale, gave a distance of the knife edges free from any error which would be occasioned by a want of parallelism.

The knife edges bisecting the cross threads of the micro-

scopes, pieces of black paper were slid beneath them, when they appeared to start forwards towards each other, the images continuing perfectly sharp and well defined.

The distance between the knife edges appeared to be now considerably less than before; and it remained to determine the difference, in order to apply its half, as a correction to the distance first obtained.

For this purpose, the reading of the micrometer was taken when the knife edges were viewed as dark objects on a white ground, and also when they were seen as light objects on a black ground. The difference of such readings will obviously give double the correction required. The results are contained in the following table.

Divisions of the microm. the ground being *white*.	Divisions of the microm. the ground being *black*.	Difference.
82,0	44,0	12,00
19,5	80,0	10,50
17,5	28,0	10,50
16,5	27,7	11,20
12,5	25,0	12,50
12,5	22,0	9,50
12,0	23,0	11,00
10,0	21,0	11,00
9,7	18,0	8,30
5,5	19,0	13,50
5,7	16,5	10,80
5,0	16,5	11,50
	Mean	11,08

From the above table it seems that 5,51 divisions (or ,000236 of an inch) are to be subtracted from the distance

obtained when the knife edges are viewed as dark objects on a light ground; and on the contrary, the same quantity to be added when they are seen as light objects on a dark ground.

From the few experiments I have made, this quantity appears to be the same, whatever may be the relative illumination of the object and its ground, so long as the difference of character is preserved. On the cause of this extraordinary fact I can hazard no conjecture, and it remains an interesting subject for future investigation.

Of the expansion of the pendulum.

The composition of brass is so various, that probably no two specimens possess precisely the same rate of expansion. It became therefore necessary to determine the expansion of the pendulum by direct experiment, instead of adopting the conclusions of others, and for this purpose the following method was used. A trough of deal was made of a length sufficient to receive the bar intended for the pendulum, which was placed edgewise in the middle of the trough, being secured at one end by wedges on both sides. The bar was supported on small pieces of glass tube, serving as rollers to prevent friction, and the trough was of the same depth as the width of the bar.

Two transverse lines were drawn near the extremities of the edge of the bar, distant from each other 49,5 inches, and a third line was subsequently drawn one inch beyond. The microscopes were placed over the lines, and left, together with a thermometer, for twenty four hours previous to the experiment.

The temperature being then registered, and the micro-

scopes having been examined to see that the lines bisected the angles formed by the spider's threads, the trough was filled with hot water to the edge of the bar, and two thermometers were placed in it, one just beneath the surface of the water, and the other at the bottom of the trough. The bar rapidly expanded, and the line on it was followed by the micrometer till it became stationary. The bisection was then perfected, and the mean of the degrees shewn by the thermometers registered, together with the number of revolutions and parts made by the micrometer. The whole was now suffered to remain till the temperature had become several degrees lower, when the contraction of the bar, occasioned by such decrease of temperature was measured, and thus several successive observations were made, which are contained in the following table.

Highest Temp.	Lowest Temp.	Diff of Temp.	Diwis. of the micrometer.	Expansion in parts of the length for each degree.
Distance between the lines on the bar, 49,5 inches.				
° 96	° 43	° 53	620	,000010116
93	43	50	580	,000010030
Distance between the lines on the bar, 50,5 inches.				
91	43	48	600	,000010616
89	84	5	70	,000011890
83	75	8	89	,000009448
75	61	14	149	,000009038
80	44	36	400	,000009486
80	60	20	215	,000009129
78	60	18	152	,000009980
		Mean of the whole,		,000009959

The mean, ,000009959 may be taken as the expansion of the pendulum in parts of its length due to a change of temperature of one degree of the thermometer.

Of the method of deducing the length of the pendulum vibrating seconds.

The distance between the knife edges was taken when the standard scale and the pendulum were both of the same temperature; and as this temperature did not differ considerably from 62°, the difference in the rate of the expansion (if any) between the pendulum and the scale may be neglected as perfectly insensible, and 62° be considered as the temperature of measurement.

The number of vibrations made by the pendulum in 24 hours, having been determined at a different temperature, the length of the pendulum will be greater or less as the temperature of observation exceeds or falls short of 62°; and by applying the expansion due to such difference of temperature, derived from the experiments contained in the preceding article, the distance of the knife edges, or length of the pendulum will be known for the temperature at which the number of vibrations was determined, whence the length of the pendulum vibrating seconds may be readily deduced, the lengths of pendulums being to each other inversely in the duplicate ratio of the number of their vibrations in 24 hours.

Of the correction for the buoyancy of the atmosphere.

The length of the pendulum thus found, differing from what it would have been had the vibrations been made in

vacuo, it is necessary to apply to it a correction for the buoyancy of the atmosphere.

For this correction, the weight of the pendulum, compared with that of air, at the time of observation, must be known.

The pendulum being composed of different kinds of brass, the specific gravity of each part was carefully determined, and from thence the specific gravity of the whole mass.

Part of the pendulum.	Weight in air.	Specific gravity.
	lb.	
3 weights (cast brass)	3,14	8,417
4 knee pieces (cast brass)	3,13	7,816
Bar (plate brass)	3,80	8,582

From the above data, the specific gravity of the pendulum is 8,469; or the weight of the pendulum compared with water is as 8,469 to 1.

It has been determined by Sir GEORGE SHUCKBURGH (Phil. Trans. for 1777) that water is 836 times heavier than air, when the thermometer is at 58°, and the barometer at 29,97 inches. But the specific gravity of air varies directly as the height of the barometer, and inversely as its expansion, which is known to be $\frac{1}{480}$th part of its bulk for each degree of FAHRENHEIT; consequently, for any other state of the barometer and thermometer, the number 836, will vary *inversely* as the height of the barometer, and *directly* $\frac{1}{480}$th part for each degree of the thermometer above 58°.

Thus the specific gravity of water, compared with that of air, may be known for the temperature and altitude of the barometer at the time of observation; and multiplying this

by the specific gravity of the pendulum, the ratio of the weight of the pendulum compared with that of air will be obtained.

This ratio will express the diminution of the force of gravity arising from the buoyancy of the atmosphere; and in order that the number of vibrations may be the same in vacuo as in air, the length of the pendulum must be increased in the proportion of this ratio to 1, the lengths of pendulums vibrating in the same time, varying directly as the force of gravity.

Detail of the experiments.

In the first experiments which were made with the pendulum, it has been already observed that the knife edges rested on plates of hard steel, but as these at the conclusion were found to have suffered penetration in no slight degree, planes of agate were substituted for them, and the results having thus been rendered doubtful, were deemed inadmissible. It may not however be irrelevant to remark, that the distances of the knife edges obtained by the two methods which have been before described, did not differ quite one ten thousandth of an inch; and, that on re-measurement after the knife edges had been used a very considerable time, their distance was found to be increased by wear, four divisions only of the micrometer, or not quite two ten thousandths of an inch. The length of the seconds pendulum deduced from these first experiments, differed from the result of the observations about to be detailed, only two ten thousandths of an inch *in defect*. I nevertheless think it useless to insert these first experiments, as the near approximation of the result cannot but be deemed to have been in some degree accidental.

In repairing the knife edges after the termination of the first series of experiments, one of them was broken, and when it was replaced by another, the distance between them was increased about one hundredth of an inch, a circumstance which proved rather gratifying than otherwise, as it afforded a pendulum differing in length from the former one, and which yet gave nearly the same result.

June 9th, 1817, the knife edges being adjusted parallel to each other, and the scale and pendulum having remained together for several preceding days, the pieces A, a, and B, b, were applied to the knife edges in the manner described in the former part of this paper, and the following measurements were taken.

Distance from A to a, 329,06 divisions. B to b, 366,97				
Date.	Readings of the micrometer.			Divisions + 39,4 in.
	A to a.	B to b.	Scale.	
June 9th.	27,0	62,0	653,0	956,51
	21,0	52,0	642,7	954,21
	13,0	52,0	642,5	958,01
	12,0	48,0	638,5	956,51
	18,0	50,0	643,0	957,01
	18,0	50,0	642,0	956,01
The pieces changed.				
10th.	65,5	112,7	698,0	956,91
	64,0	112,5	696,0	955,76
	61,0	106,2	693,2	957,61
	64,5	108,0	693,5	955,26
	65,0	106,2	694,5	956,91
	67,2	107,0	696,0	956,91
Mean of the whole				956,47

Hence the distance between the knife edges is 39,4 inches +956,47 divisions of the micrometer.

June 12th, the knife edges having been adjusted parallel to each other, the following measurements were taken, the knife edges being viewed as *dark* objects on a *white* ground.

Dark on a white ground.

Date.	Readings of the micrometer. Near side of the bar.	Further side of the bar.	Mean.	Scale.	Divisions +39,4 in.
June 12	50,0	50,0	50,00	1006,5	956,50
	50,0	50,0	50,00	—	956,50
	50,0	50,0	50,00	—	956,50
	49,0	50,0	49,50	—	956,50
12	46,5	44,0	42,25	1001,0	955,75
	44,5	44,5	45,50	1001,0	955,50
	42,0	43,0	42,50	1003,0	960,50
	43,0	43,0	43,00	1003,0	960,00
13	37,5	38,0	37,75	994,0	956,25
	35,0	39,0	37,00	993,5	956,50
	38,0	35,0	36,50	1001,0	964,50
	38,0	38,0	38,00	—	963,00
14	25,5	27,5	26,50	987,0	960,50
	25,0	26,5	25,75	987,5	961,75
	24,0	25,2	24,60	—	962,90
	25,0	25,7	25,25	—	962,25
14	79,5	78,0	78,75	1042,0	963,25
	76,0	75,0	75,50	—	966,50
	72,0	71,5	71,75	1035,2	963,45
	74,0	73,5	73,75	—	961,45
				Mean of the whole	960,00
				Correction for irradiation (see page 59)	−5,51
					954,49

By the foregoing measurements, the distance between the knife edges appears to be 39,4 inches +954,49 divisions.

The pendulum was now placed on its support, and the following experiments made for equalising the number of vibrations.

Slider 18 divisions. Clock losing 0",33 on mean time. — Great weight *above*. — Barometer 29,7.

	Temp.	Time of coincidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
	°	m. s.	°	°				s	
June 19th.	66,8	29.12	1,29	1,18	518	516	86066,40	2,28	86068,68
		37.50	1,07	1,00	520	518	86067,70	1,63	86069,33
	66,8	46.30	0,93						
								Mean	86069,00
								Clock	— 0,33
	66,8	mean							86068,67
		Great weight *below*.							
	66,6	5,59	1,25	1,19	513	511	86063,16	2,32	86065,48
		14,32	1,13	1,06	512	510	86062,50	1,84	86064,34
	66,7	23,4	1,01						
								Mean	86064.91
								Clock	— 0,33
								Temp.	— 0,04
	66,7	mean							86064,54

Here the vibrations were in excess; the slider was therefore placed at 29 divisions, and the second weight moved nearer to its knife edge.

Slider 29 divisions. Clock losing 0″,33 on mean time.		Great weight *above*. Second weight moved.							Barometer 29,7.
	Temp.	Time of coincidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
	°	m. s.	°	°				s	
June 19th	67,0	57.42	1,23	1,13	506	504	86058,49	2,08	86060,57
		6. 8	1,04	0,96	509	507	86060,50	1,51	86062,01
	66,9	14.37	0,88						
								Mean	86061,29
								Clock	— 0,33
	66,9	mean.							86060,96
Great weight *below*.									
	67,1	24.39	1,14	1,07	506	504	86058,49	1,87	86060,36
		33. 5	1,01	0,97	507	505	86059,16	1,51	86060,67
	67,1	41.32	0,93						
								Mean	86060,51
								Clock	—0,33
								Temp.	+0,09
	67,1	mean.							86060,27

The number of vibrations being still in excess, the second weight was moved again, the slider remaining as before.

Slider 29 divisions. Clock losing 0″.33 on mean time.		Great weight *above*. Second weight moved.						Barometer 29,7.	
	Temp	Time of coincidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
	°	m. s.	°	°				s.	
June	67,3	51. 7	1,21	1,11	503	501	86056,47	2,02	86058,49
19th		59 30	1,02	0,95	504	502	86057,16	1,48	86058,64
		7.54	0,88	0,80	506	504	86058,49	1,05	86059,54
		16.20	0,73	0,68	507	505	86059,16	0,75	86059,91
	67,3	24.47	0,63						
								Mean	86059,14
								Clock	— 0,33
	67,3	mean							86058,81
		Great weight *below*.							
	67,4	30. 8	1,14	1,09	504	502	86057,16	1,94	86059,10
		38 32	1,05	0,99	504	502	86057,16	1,60	86058,76
		46 56	0.94	0,89	506	504	86058,49	1,30	86059,79
		55.22	0,84	0,79	506	504	86058,49	1,02	86059,51
	67,4	3.48	0,75						
								Mean	86059,29
								Clock	— 0,33
								Temp.	+ 0,04
	67,4	mean							86059,00

Slider 29 divisions.
Clock losing 0",26 on mean time.

Great weight *above*.

Barometer 29,76

	Temp.	Time of coincidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
	°	m. s.	°	°				s.	
June 20th.	68,7	1.17	1,22	1,13	503	501	86056,47	2,08	86058,55
		9.40	1,04	0,96	503	501	86056,47	1,51	86057,98
		18. 3	0,88	0,80	505	503	86057,82	1,05	86058,87
		26.28	0,73	0,68	504	502	86057,16	0,75	86057,91
	68,7	34.52	0,63						
								Mean	86058,33
								Clock	— 0,26
	68,7	mean							86058,07

Great weight *below*.

	68,4	22.4	1,19	1,13	504	502	86057,16	2,08	86059,24
		30.28	1,07	1,00	504	502	86057,16	1,63	86058,79
		38.52	0,94	0,90	504	502	86057,16	1,38	86058,54
		47.16	0,86	0,82	505	503	86057,82	1,10	86058,92
	68,5	55.41	0,78						
								Mean	86058,87
								Clock	— 0,26
								Temp.	— 0,09
	68,5	mean							86058,52

The number of vibrations being now in both positions sufficiently near each other, and in *defect*, the second weight was secured in its place.

Slider 23 divisions.
Clock losing 0″,30 on mean time.

Great weight *above*.

Barometer. 29,76.

	Temp.	Time of coincidence.	Arc of vibration	Mean arc.	Interv. in seconds	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
	°	m. s.	°	°				s.	
June	68,7	48.25	1,23	1,13	504	502	86057,16	2,08	86059,24
20th.		56.49	1,03	0,97	506	504	86058,49	1,54	86060,03
		5.15	0,91	0,82	507	505	86059,16	1,10	86060,26
		13.42	0,73	0,68	506	504	86058,49	0,75	86059,24
	68,7	22.08	0,64						
								Mean	86059,69
								Clock	— 0,30
	68,7	mean							86059,39

A

Great weight *below*.

68,8	31.17	1,22	1,16	504	502	86057,16	2,20	86059,36
	39.41	1,10	1,04	505	503	86057,82	1,76	86059,58
	48.6	0,99	0,94	506	504	86058,49	1,44	86059,93
	56.32	0,90	0,84	506	504	86058,49	1,15	86059,64
68,9	4.58	0,79						
							Mean	86059,63
							Clock	— 0,30
							Temp.	+ 0,09
68,9	mean							86059,42

Slider 23 divisions. Clock losing 0",20 on mean time. **Great weight *above*.** Barometer 29,86.

	Temp.	Time of coincidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
	°	m. s.	°	°				s	
June	71,2	54. 8	1,23	1,13	503	501	86056,47	2,08	86058,55
21st.		2.31	1,03	0,96	502	500	86055,78	1,15	86056,93
		10.53	0,89	0,81	504	502	86057,16	1,07	86058,23
		19.17	0,73	0,68	504	502	86057,16	0,75	86057,91
	71,3	27.41	0,63						
								Mean	86057,90
								Clock	— 0,20
	71,3	mean							86057,70
B		**Great weight *below*.**							
	71,0	12.52	1,20	1,14	503	501	86056,47	2,12	86058,59
		21.15	1,08	1,02	503	501	86056,47	1,70	86058,17
		29.38	0,97	0,92	503	501	86056,47	1,38	86057,85
		31.1	0,88	0,83	504	502	86057,16	1,13	86058,29
	71,1	46.25	0,79						
								Mean	86058,22
								Clock	— 0,20
								Temp.	— 0,09
	71,1	mean							86057,93

Slider 23 divisions. Clock losing 0″,20 on mean time		Great weight *above*.						Barometer 29,86.
	Temp.	Time of coincidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours	Corr. for arc.	Vibrations in 24 hours.
	°	m. s.	°					s.	
June	71,4	36.6	1,22	1,12	502	500	86055,78	2,05	86057,83
21st.		44.28	1,03	0,95	503	501	86056,47	1,48	86057,95
		52.51	0,87	0,80	503	501	86056,47	1,04	86057,51
		1.14	0,73	0,67	506	504	86058,49	0,73	86059,22
	71,4	9.40	0,62					Mean	86058,13
								Clock	— 0,20
	71,4	mean							86057,93
C.		Great weight *below*.							
	71,5	20.24	1,19	1,13	502	500	86055,78	2,08	86057,86
		28.46	1,07	1,00	503	501	86056,47	1,63	86058,10
		37. 9	0,94	0,89	503	501	86056,47	1,29	86057,76
		45.32	0,85	0,81	503	501	86056,47	1,07	86057,54
	71,6	53.55	0,78					Mean	86057,81
								Clock	— 0,20
								Temp.	+ 0,09
	71,6	mean							86057,70

Slider 23 divisions. Clock gaining 0″,30 on mean time.		Great weight *above*.						Barometer 29,95.	
	Temp.	Time of coincidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
	°	m. s.	°	°				s	
June	73,0	9. 8	1,22	1,12	500	498	86054,40	2,05	86056,45
23rd.		17.28	1,02	0,94	501	499	86055,09	1,44	86056,53
		25.49	0,87	0,80	501	499	86055,09	1,04	86056,13
		34.10	0,74	0,68	501	499	86055,09	0,75	86055,84
	73,1	42.31	0,63						
								Mean	86056,24
								Clock	+ 0,30
	73,1	mean							86056,54
D.		Great weight *below*.							
	72,4	27.33	1,21	1,15	501	499	86055,09	2,16	86057.25
		35 54	1,09	1,04	501	499	86055,09	1,76	86056,85
		44.15	0,99	0.94	501	499	86055,09	1,44	86056,53
		52.36	0,89	0,84	502	500	86055,78	1,15	86056,93
	72,8	0 58	0,79						
								Mean	86056,89
								Clock	+ 0,30
								Temp.	— 0,22
	72,6	mean.							86056,97

The pendulum was now taken down to re-measure the distance between the knife edges, in order to ascertain whether or not they had suffered from use.

The pieces A, a, B and b, being applied as before, the following measurements were taken.

Distance from A to a, 329,06 divisions. B to b, 366,97.				
Date.	Readings of the micrometer.			Divisions. + 39.4 in.
	A to a.	B to b.	Scale.	
June 25th.	9,7	39,0	630,0	953,66
	7,0	37,3	630,0	955,86
	10,0	36,5	630,7	955,46
The pieces made to change places				
26th.	59,0	87,0	680,0	955,01
	59,0	84,0	680,3	956,81
	51,0	75,0	671,0	955,51
	43,0	67,7	664,5	957,16
	41,5	68,0	662,5	955 76
			Mean of the whole	955,65

Hence the distance between the knife edges is 39,4 inches +955,65 divisions of the micrometer.

Having thus satisfied myself that no injury to the knife edges was to be apprehended from moderate use, the pendulum was again suspended, but now, to my surprise, I found the number of vibrations different from what they were before the re-measurement. This difference became still greater on the following day, and it at length occurred to me that the moisture of the atmosphere must have undergone some change, and that an alteration had been thus occasioned in the weight of the wooden extremities of the pendulum. On referring to the register of the hygrometer

kept by Mr. BROWNE, it was found that a considerable change had in fact suddenly taken place from moisture to dryness; and, so great was the derangement of the pendulum from this apparently trivial cause, that it became necessary to move the second weight. This was accordingly done, and the following experiments made for again bringing the number of vibrations to an equality.

Slider 29 divisions. Clock gaining 0″,18 on mean time. — Great weight *above*. Second weight moved. — Barometer 29,70.

	Temp.	Time of coincidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
		m. s.	°					s.	
July 1st.	69,1	10.31	1,21	1,11	501	499	86055,09	2,01	86057,10
		18.52	1,01	0,92	502	500	86055,78	1,38	86057,16
	69,1	27.14	0,83						
								Mean	86057,13
								Clock	+ 0,18
	69,1	mean.							86057,31
		Great weight *below*.							
	68,8	47.22	1,23	1,16	502	500	86055,78	2,20	86057,98
		55.44	1,09	1,04	502	500	86055,78	1,76	86057,54
	68,9	4. 6	0,99						
								Mean	86057,76
								Clock	+ 0,18
								Temp.	— 0,09
	68,9	mean							86057,85

The second weight was now securely fixed.

Slider 19 divisions.
Clock gaining 0″,18 on mean time.

Great weight *above*.

Barometer 29,70.

	Temp.	Time of coincidence.	Arc of vibration.	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
	°	m. s.	°	°				s.	
July 1st.	69,3	0.52	1,21	1,10	505	503	86057,82	1,98	86059,80
		9.17	1,00	0,93	505	503	86057,82	1,42	86059,24
	69,2	17.42	0,86					Mean	86059,52
								Clock	+ 0,18
	69,3	mean							86059,70

Great weight *below*.

	69,3	22.57	1,25	1,19	504	502	86057,16	2,31	86059,47
		31.21	1,13	1,06	504	502	86057,16	1,83	86058,99
	69,3	39.45	1,00						
								Mean	86059,23
								Clock	+ 0,18
	69,3	mean							86059,41

Slider 21 divisions. Clock gaining 0″,18 on mean time. — Great weight *above*. — Barometer 29,70.

	Temp.	Time of coincidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours	Corr. for arc.	Vibrations in 24 hours.
	°	m. s.	°	°				°	
July 1st.	69,3	28. 9	1,23	1,13	503	501	86056,47	2,08	86058,55
		36.32	1,03	0,96	504	502	86057,16	1,51	86058,67
		44.56	0,89	0,81	505	503	86057,82	1,07	86058,89
	69,3	53.21	0,74					Mean	86058,70
								Clock	+ 0,18
	69,3	mean							86058,88
E.		Great weight *below*.							
	69,3	49. 6	1,21	1,15	503	501	86056,47	2,16	86058,63
		57.29	1,09	1,03	504	502	86057,16	1,73	86058,89
		5.53	0,98	0,93	504	502	86057,16	1,41	86058,57
	69,3	14.17	0,88	0,83	505	503	86057,82	1,13	86058,95
		22.42	0,78					Mean	86058,76
								Clock	+ 0,18
	69,3	mean.							86058,94

Slider 20 divisions.
Clock gaining 0″,18 on mean time.

Great weight *above*.

Barometer 29,70.

	Temp.	Time of coincidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
July 1st.	°	m. s.	°	°				s.	
	69,3	1.46	1,34	1,23	502	500	86055,78	2,47	86058,25
		10. 8	1,12	1,03	503	501	86056,47	1,73	86058,20
		18.31	0,94	0,86	505	503	86057,82	1,21	86059,03
	69,3	26.56	0,79	0,73	506	504	86058,49	0,87	86059,36
								Mean	86058,71
								Clock	+ 0,18
	69,3	mean							86058,89

F.

Great weight *below*.

	69,3	40.28	1,23	1,17	503	501	86056,47	2,23	86058,70
		48.51	1,11	1,04	503	501	86056,47	1,77	86058,24
		57.14	0,98	0,93	504	502	86057,16	1,41	86058,57
	69,4	5.38	0,88	0,84	506	504	86058,49	1,15	86059,64
		14. 4	0,80						
								Mean	86058,79
								Clock	+ 0,18
								Temp.	+ 0,04
	69,4								86059,01

Slider 20 divisions.
Clock gaining 0",18 on mean time.

Great weight *above*.

Barometer. 29,70.

	Temp.	Time of coincidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrati.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
July	°	m. s.	°	°				".	
2nd.	68,5	33. 7	1,41					2,72	86058,50
		41.29	1,18	1,29	502	500	86055,78	1,87	86059,69
		49.54	0,97	1,07	505	503	86057,82	1,29	86058,45
		58.18	0,82	0,89	504	502	86057,16	0,94	86058,76
	68,5	6.43	0,71	0,76	505	503	86057,82		
								Mean	86058,85
								Clock	+ 0,18
	68,5	mean							86059,03
G.		Great weight *below*.							
	68,0	51.20	1,17					1,98	86059,14
		59.44	1,04	1,10	504	502	86057,16	1,44	86059,93
		8.10	0,94	0,94	506	504	86058,49	1,29	86058,45
		16.34	0,84	0,89	504	502	86057,16	1,02	86059,51
	68,0	25. 0	0,75	0,79	506	504	86058,49		
								Mean	86059,26
								Clock	+ 0,18
								Temp.	— 0,22
	68,0	mean							86059,22

Slider 18 divisions.
Clock gaining 0",18 on mean time.

Great weight *above*.

Barometer 29,70.

	Temp.	Time of coincidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrat.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
	°	m. s.	°	°				s.	
July 2nd.	68,6	18.31	1,25	1,14	504	502	86057,16	2,12	86059,28
		26.55	1,04	0,96	504	502	86057,16	1,50	86058,66
		35.19	0,89	0,81	507	505	86059,16	1,07	86060,23
	68,7	43.46	0,74	0,68	505	503	86057,82	0,75	86058,57
								Mean	86059,18
								Clock	+ 0,18
	68,7	mean							86059,36
H.		Great weight *below*.							
	68,8	57.41	1,23	1,16	503	501	86056,47	2,20	86058,67
		6. 4	1,10	1,05	504	502	86057,16	1,80	86058,96
		14.28	1,00	0,94	504	502	86057,16	1,44	86058,60
	69,1	23.52	0,88	0,85	506	504	86058,49	1,18	86059,67
								Mean	86058,98
								Clock	+ 0,18
								Temp.	+ 0,09
	68,9	mean							86059,25

Slider 18 divisions. Clock gaining 0″,18 on mean time. **Great weight *above*.** Barometer. 29,70.

	Temp.	Time of coincidence.	Arc of vibration	Mean arc.	Interv. in seconds	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
	°	m. s.	°					s.	
July 2nd.	69,4	17.43	1,24	1,13	504	502	86057,16	2,08	86059,24
		26. 7	1,03	0,95	504	502	86057,16	1,47	86058,63
		34.31	0,88	0,81	504	502	86057,16	1,07	86058,23
		42.55	0,74	0,69	507	505	86059,16	0,78	86059,94
	69,3	51.22	0,64						
								Mean	86059,01
								Clock	+ 0,18
	69,3	mean							86059,19
I.		**Great weight *below*.**							
	69,3	38.22	1,30	1,14	502	500	86055,78	2,12	86057,90
		46.44	1,08	1,03	505	503	86057,82	1,73	86059,55
		55. 9	0,98	0,93	504	502	86057,16	1,42	86058,58
		3.33	0,88	0,84	505	503	86057,82	1,15	86058,97
	69,3	11.58	0,80						
								Mean	86058,75
								Clock	+ 0,18
	69,3	mean							86058,93

Slider 18 divisions.
Clock gaining 0″,18 on mean time.

Great weight *above*.

Barometer 29,70.

	Temp.	Time of co-incidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrat.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
	°	m. s.	°	°				s	
July	69,3	53.29	1,31	1,19	503	501	86056,47	2,31	86058,78
and.		1.52	1,08	1,00	504	502	86057,16	1,63	86058,79
		10.16	0,92	0,84	506	504	86058,49	1,15	86059,64
		18.42	0,76	0,71	505	503	86057,82	0,82	86058,64
	69,3	26. 7	0,66					Mean	86058,96
								Clock	+ 0,18
	69,3	mean							86059,14

K.

Great weight *below*.

	69,3	32.18	1,24	1,17	502	500	86055,78	2,23	86058,01
		40.40	1,10	1,05	504	502	86057,16	1,80	86058,96
		49. 4	1,00	0,95	505	503	86057,82	1,47	86059,29
		57.29	0,90	0,85	504	502	86057,16	1,18	86058,34
	69,3	5.53	0,81					Mean	86058,65
								Clock	+ 0,18
	69,3	mean							86058,83

Slider 19 divisions.
Clock gaining 0".18
on mean time.

Great weight *above*.

Barometer
29,90.

	Temp.	Time of coincidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
	°	m. s.	°	°				s.	
July 3rd.	68,0	2.18	1,28	1,17	504	502	86057,16	2,23	86059,39
		10.42	1,06	0,98	504	502	86057,16	1,57	86058,73
		19. 6	0,91	0,82	505	503	86057,82	1,10	86058,92
		27.31	0,74	0,69	506	504	86058,49	0,77	86059,26
	68,2	35.57	0,64						
								Mean	86059,08
								Clock	+ 0,18
	68,1	mean							86059,26

L.

Great weight *below*.

	67,7	23.52	1,29	1,21	504	502	86057,16	2,39	86059,55
		33.16	1,14	1,07	504	502	86057,16	1,87	86059,03
		40.40	1,01	0,97	504	502	86057,16	1,54	86058,70
		49. 4	0,93	0,82	506	504	86058,49	1,10	86059,59
	67,8	57.30	0,72						
								Mean	86059,22
								Clock	+ 0,18
								Temp.	— 0,18
	67,8	mean							86059,22

Slider 19 divisions.
Clock gaining 0″,18 on mean time.

Great weight *above*.

Barometer 29,90.

	Temp.	Time of co-incidence.	Arc of vibration	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
		m. s.	°	°				s	
July 3rd.	68,3	45. 3	1,23	1,13	504	502	86057,16	2,08	86059,24
		53.27	1,03	0,95	504	502	86057,16	1,47	86058,63
		1.51	0,87	0,80	505	503	86057,82	1,04	86058,86
		10.16	0,74	0,68	506	504	86058,49	0,75	86059,24
	68,4	18.42	0,63						
								Mean	86058,99
								Clock	+ 0,18
	68,4	mean							86059,17

M.

Great weight *below*.

	68,4	24.31	1,24	1,17	503	501	86056,47	2,23	86058,70
		32.54	1,11	1,05	504	502	86057,16	1,80	86058,96
		41.18	0,99	0,94	504	502	86057,16	1,44	86058,60
		49.42	0,90	0,86	506	504	86058,49	1,20	86059,69
	68,5	58. 8	0,82						
								Mean	86058,99
								Clock	+ 0,18
								Temp.	+ 0,04
	68,5	mean							86059,21

The results of such of the preceding experiments as are to be used for calculating the length of the seconds pendulum, are brought under one view in the following table:

Place of the slider	Expt.	Temp.	Barom.	No. of vibrations. Great wt. above.	Diff.	No. of vibrations. Great wt. below.	Vibs. in excess or defect.
23	A	68,7	29,76	86059,39	,03	86059,42	—
23	B	71,3	29,86	86057,70	,23	86057,93	—
23	C	71,4	29,86	86057,93	,23	86057,70	+
23	D	73,1	29,95	86056,54	,43	86056,97	—
Pendulum re-measured.							
21	E	69,3	29,70	86058,88	,06	86058,94	—
20	F	69,3	29,70	86058,89	,12	86059,01	—
20	G	68,5	29,70	86059,03	,19	86059,22	—
18	H	68,7	29,70	86059,36	,11	86059,25	+
18	I	69,3	29,70	86059,19	,16	86058,93	+
18	K	69,3	29,70	86059,14	,31	86058,83	+
19	L	68,1	29,90	86059,26	,04	86059,22	+
19	M	68,4	29,90	86059,17	,04	86059,21	—
			Mean	86058,71		86058,72	

No other explanation of this table appears to be necessary, than that the column entitled "Difference" expresses the difference between the number of vibrations in the two positions of the pendulum, and that the last column indicates by the sign + or — whether the number of vibrations exceeds or falls short of the truth; which inference is drawn from a comparison of the number of vibrations when the great weight is above, with the number in that position of the pendulum when the great weight is below. The mean of the vibrations in the column "Great weight *above*" not differing sensibly from that headed "Great weight *below*," is a proof that the number of vibrations in either position of the pendulum may be considered as equal, and consequently that the

one knife edge being the point of suspension, the other must necessarily be in the centre of oscillation.

Length of the pendulum vibrating seconds.

The distance between the knife edges was as follows:

	Inches.	Divisions.	Inches.
By the 1st measurement	39.4 +	956,47	= 39,44094
By the 2nd, -	39.4 +	954,49	= 39,44086
By the 3d, -	39.4 +	955,65	= 39,44090
		Mean	39,44090
Corr. for error in division of the scale (see page 54) -	-		— 0,00005
			39,44085

Hence, 39,44085 inches may be taken as the distance between the knife edges at the temperature of 62 degrees.

Using the vibrations when the great weight was *below*, as being nearer to the truth than in the other position of the pendulum, we obtain the following results.

Expt.	Temp.	Barom.	Vibrations in 24 hours.	Length of the seconds pen. in air	Corr. for the atmosphere.	Length of the sec. pend. in vacuo.	Difference from the mean.
A	68,7	29,76	86059,42	39,13313	,00544	39,13857	+,00028
B	71,3	29,86	86059,93	39,13278	,00544	39,13822	—,00007
C	71,4	29,86	86057,70	39,13260	,00544	39,13804	—,00025
D	73,1	29,95	86056,97	39,13259	,00544	39,13803	—,00026
E	69,3	29,70	86058,94	39,13293	,00544	39,13837	+,00008
F	69,3	29,70	86059,01	39,13298	,00544	39,13842	+,00013
G	68,5	29,70	86059,32	39,13286	,00545	39,13831	+,00002
H	68,7	29,70	86059,25	39,13296	,00544	39,13840	+,00011
I	69,3	29,70	86058,93	39,13291	,00544	39,13834	+,00005
K	69,3	29,70	86058,83	39,13283	,00544	39,13825	—,00003
L	68,1	29,90	86059,21	39,13271	,00548	39,13819	—,00009
M	68,4	29,90	86059,21	39,13281	,00548	39,13829	—,00000
					Mean	39,13829	

The length of the pendulum thus obtained requires yet another correction to reduce it to what it would have been at the level of the sea. The elevation of the apartments of the Royal Society at Somerset House above low-water mark, is known to be 81 feet; and by several careful observations with an excellent mountain barometer by RAMSDEN, I found the room in Portland Place, in which the experiments were made, to be two feet below the Royal Society's apartments; and as the height of the pendulum above the floor was four feet, we obtain 83 feet for the elevation of the pendulum above the level of the sea. Now the force of gravity increasing inversely as the square of the distance from the earth's centre, the length of the pendulum must be increased in the same proportion, and taking the radius of the earth for the latitude of Portland Place to be 3954,583 miles, we have 39,1386 inches for the length of the pendulum vibrating seconds at the level of the sea.

It may be remarked that the greatest difference between the mean result and that of any one of the twelve sets of experiments contained in the preceding table, is only ,00028 of an inch, or $\frac{1}{1349\frac{1}{2}}$ of the whole length of the pendulum; and as seven of the twelve sets are within one ten thousandth of an inch of the mean result, it may be inferred that the above determination cannot be very distant from the truth.

The length here given, is that required to perform one vibration in $\frac{1}{86400}$ part of a mean *solar* day, this being the measure of time usually employed for the purpose; but I am at a loss to conjecture why this is preferred to the sidereal day, a measure of time which marks a complete revolution

of the earth, and is readily obtained, being the interval between the returns of any fixed star to the meridian.

I shall now proceed to notice the sources of error which may be supposed to have affected the results of the preceding experiments.

These may be classed under the following heads.

1. The measurement of the distance of the knife edges.
2. The number of vibrations in 24 hours.
3. The temperature, and
4. The form of the knife edges.

On the first, it is scarcely necessary to offer any remark. Since the mean results of three several sets of measurements are within one ten-thousandth of an inch of each other, and the different methods employed, preclude, it may be presumed, any accidental coincidence, we may with confidence infer that the error in the distance of the knife edges, cannot amount to one ten-thousandth of an inch.

Among the number of vibrations in 24 hours given in the various sets of experiments, there appear to be differences which amount in some instances to 1,6. These differences however do not influence the truth of the result, beyond a certain minute quantity, the extent and origin of which I shall proceed to explain.

In order to determine the vibrations in 24 hours, it is necessary to ascertain the number of vibrations and parts of a vibration made by the brass pendulum during a certain number of *complete* seconds; but the moment of observation being limited to that when the brass pendulum is at the lowest part of the arc, the process is of necessity reversed, and the brass pendulum is observed to make a certain number of *complete* vibrations,

during a certain number of seconds and parts of a second which constitute *the interval*. The disappearance of the disk can however be noted only to a single second, and the brass pendulum may arrive at the lowest part of the arc either precisely at this second, or at any portion of a second preceding it. An error might possibly arise from this circumstance amounting to nine tenths of a second, by which the interval deduced from observation would be less than the truth, and as an error of one second in the interval, occasions a difference of 0,63 in the number of vibrations in 24 hours, if 0,55 (the proportional part of 0,63) be divided by 4 (the number of intervals forming each set of experiments) we have 0,14 for the greatest error *in defect* in the number of vibrations in 24 hours which can arise from this cause.

On the contrary, if the *second* coincidence or return of the brass pendulum to the lowest point of the arc, should have taken place nine tenths of a second before the second at which the disappearance of the disk was noted, the error in the number of vibrations in 24 hours would amount to the same quantity, and would now be *in excess*.

If the first and third coincidences take place accurately at the time of the observed disappearance of the disk, and the observation of the second coincidence should differ nine tenths of a second from the truth, it is obvious that the number of vibrations in 24 hours deduced from each interval will be erroneous about 0,56 the one being *in excess*, the other *in defect*: The mean of both will be the truth, though the observed difference between the two amounts to so considerable a quantity as 1,2.

The last coincidence of each set, takes place when the arc

of vibration is much reduced. It is therefore not impossible that an error of one second may sometimes, though rarely, occur in determining the time of this coincidence. This would occasion an error of about 0,63 in the number of vibrations in 24 hours, which divided by 4 as before, would influence the mean result 0,15 of a vibration.

In estimating these errors, I have taken an extreme case, as it is probable they would in most instances be compensated by the succeeding intervals. Supposing them however to be combined, the greatest effect on the mean result of any one set of experiments might amount to about 0,3 of a vibration in 24 hours, and the difference between the number of vibrations in either position of the pendulum, might have been double this quantity, and yet when the great weight was below, not have differed from the truth more than 0,3 of a vibration.

It appears then, that if the experiments have been conducted with sufficient care, no greater difference should be found between the mean, and any one of the resulting lengths of the pendulum contained in the preceding table, than might have been occasioned by a difference of 0,30 f a vibration in 24 hours, and this is found to be about 0,0003 of an inch.

In fact, on referring to the table we perceive that the experiments A and D, which differ most from the mean, give, the one, 00029 of an inch *in excess*, and the other ,00026 *in defect*.

In considering the sources of error, it may not be unnecessary to remark that had the bar of the pendulum been made too thick, and the knife edges not been placed accurately at right angles to it, an error, though very minute, might have arisen from the effect of the obliquity in diminishing the dis-

tance of the centre of oscillation from the axis. This was sufficiently guarded against by having the bar so thin as to ensure its becoming perpendicular by its own weight, had the position of the knife edge been in a small degree erroneous; for though the form the bar would assume is strictly speaking a curve, it may without sensible error be considered as a straight line.

With regard to temperature, every precaution was taken to prevent error. The thermometer used was made by Mr. TROUGHTON for the late Sir GEORGE SHUCKBURGH. It is divided into half degrees, and the height of the mercury may be estimated to one tenth of a degree. It has been already observed in the preceding part of this paper, that the thermometer was approached only at the first and last coincidences.

The experiments themselves afford, it is presumed, a sufficient proof of the stability of the knife edges. Every care was taken to form them in the first instance as perfect as possible; and after four sets of experiments had been made, they were found on re-measurement to have suffered no perceptible alteration; and it is evident by the near agreement of the results, that they remained uninjured during the succeeding experiments: it is difficult therefore to conceive that any error can have arisen from this source.

I may here remark, that the method I have employed in determining the length of the pendulum, possesses other advantages besides that of superseding the errors arising from unequal density or figure; and one, not the least considerable is, that after a very few vibrations, the true length of the pendulum is bounded by certain known limits. Thus in the two first sets of experiments, after the re-measurement of the

distance between the knife edges, we may remark that when the slider was at 29 divisions, the number of vibrations (the great weight being below) was, 86057,85 and in *defect*; and when the slider was removed to 19 divisions, the number of vibrations was 86059,41 and in *excess*. The true number of vibrations then is evidently between the two, and the utmost extent of error in using either of these numbers must fall short of 1,78 their difference when reduced to the same temperature. But if the mean be employed in the computation, the length of the pendulum will be found to differ only about four ten-thousandths of an inch from the mean result given in the foregoing table.

It may not be unnecessary to add that every experiment made has been retained; nor do I consider any one as less entitled to credit than the rest, excepting that marked A, in the table; and that, only because the rate of the clock was not observed on the day of the experiment, but was taken to be the same as the rate of the following day.

The length then of the pendulum vibrating seconds in vacuo at the level of the sea, measured at the temperature of 62° of FAHRENHEIT, appears to be

	inches.
By Sir G. SHUCKBURGH's standard -	39,13860
By General ROY's scale - -	39,13717
By BIRD's Parliamentary standard -	39,13842

the latitude of the place of observation being 51° 31′ 8″,4 north.*

* The latitude was deduced from the data contained in the trigonometrical survey; Mr. BROWNE's house bearing from Portland Chapel 74°. 38′. 50″. west from the north, the distance being 283 feet. This differs only 0″,1 from the latitude determined by Mr. BROWNE from a great number of observations.

An objection might be urged against the use of the knife edge, on the ground that being an elastic substance it may possibly suffer temporary compression, and thus perhaps introduce a source of error. In order to meet any doubt that might arise on this important part of the subject, it is my intention to commence a series of experiments with a pendulum of the same construction as that which has been described, but vibrating on cylinders instead of knife edges, and I trust soon to have the honor of laying the result before the Royal Society.

London, July, 1817.

APPENDIX.

Since the preceding Paper was written, a very curious and important theorem has been discovered by M. LAPLACE, of which Dr. YOUNG has favoured me with a concise demonstration, together with some other investigations which I shall subjoin in his own words.

MY DEAR SIR,

I cannot forbear to congratulate you on the discovery of the singular property of your pendulum, which has lately been demonstrated by M. LAPLACE, since it appears to remove the only doubt, that could reasonably be entertained, of the extreme accuracy of the results of your experiments. The correction for the curvature of the rolling surfaces, in the case of a simple pendulum, is very easily obtained from the geometrical determination of the curve described, although M. LAPLACE's train of reasoning, from mechanical principles, is somewhat too elaborate to be readily followed through all the symbols in which it is enveloped: and the same geometrical considerations appear, at first sight, to be equally applicable to the case of compound pendulums in general, since the motions of all their effective parts are concentric with those of a simple one similarly suspended. But upon further reflection, it becomes evident that these motions, though concentric, are related to each other in proportions somewhat different from those of a similar pendulum vibrating on a single point, and it is therefore necessary to deter-

mine the modification of the motion produced by this difference of connexion. The investigation may however be conducted in a method much more simple and intelligible to ordinary capacities, than that which has been adopted by the celebrated mathematician to whom we are indebted for the theorem; and I am tempted to send you an "*aperçu*" of the reasoning by which I have satisfied myself respecting it.

It follows immediately from the general theorem for finding the curvature of trochoids of all kinds, (Lectures on Nat. Phil. II. p. 559) that the radius of curvature of the path of any point, in the rod of a pendulum supported by a cylindrical axis, will initially be a third proportional to the distances of the point from the centre of the cylinder, and from the surface on which it rolls: so that when the cylinder is small, and the pendulum simple, the centre of curvature of its path may be considered as situated at the distance of the radius r below the point of contact: and this is obviously the only correction required for such a pendulum as that of BORDA. But when the weight is divided, or of considerable magnitude, it becomes necessary to calculate the effect of the different curvatures of the paths of its different parts, and to compare these paths with that of a pendulum A of any given length a. Supposing, for the sake of simplicity, the weight of each horizontal section to be concentrated in the vertical line, and calling the distance of any particle P below the surface of the cylinder x, the radius of curvature of its path will be a third proportional to $x+r$ and x, that is, $\frac{xx}{x+r}$; and the inclination of the curve at a given distance from the vertical line being always directly as the curvature, or inversely as its radius, the force derived from the weight of P will be

to the force at an equal distance in the path of A, as a to $\frac{xa}{x+r}$, or as $\frac{a(x+r)}{xx}$ to 1. Now the point of the rolling pendulum confined to the vertical line is not the centre of curvature, but initially the surface of the cylinder: so that this must be considered as the point of intersection with the vertical line, and as the fulcrum of the lever; consequently the distance of P from the vertical line will be, to that of the pendulum A, as x to a, and its immediate force will be $\frac{a(x+r)}{xx} \cdot \frac{x}{a} \cdot P = \frac{x+r}{x} P$; but this force, acting only at the end of a lever x, will have its effect at A again reduced in the ratio of x to a, and will then become $\frac{x+r}{a} P$: and if we express the sum of all the similar forces belonging to the body by the character Σ, whether found by a fluxional calculation or otherwise, we have the whole force, at A, $\Sigma \frac{x+r}{a} P$. The reduced or rotatory inertia of the body, sometimes very improperly called the "momentum" of inertia, will also be expressed by $\Sigma \frac{xx}{aa} P$, being reduced in the ratio of the squares of the distances from the fulcrum; consequently the accelerative force will be to that of the pendulum A as $\frac{\Sigma \frac{xx}{aa} P}{\Sigma \frac{x+r}{a} P}$ to 1, or as $\frac{\Sigma xx P}{a \Sigma (x+r) P}$ to 1; since it is indifferent whether the integral or the differential be divided by the constant quantity a: and in order to express the length of the equivalent pendulum, we must suppose a to be as much lengthened as the force is weakened, so that we have for this length $\frac{\Sigma xx P}{\Sigma (x+r) P}$. It is obvious that the denominator of this fraction is the same that would express the force of the body with regard to the centre of the cylinder as a fixed point; and it might indeed

have been inferred at once, from the principle of virtual velocities, that the force must be the same in either case, however irregular the form of the body may be: but it is somewhat more satisfactory to follow the mechanical steps by which the operation of the law takes place. If we make $r=0$, we have $\frac{\Sigma x x P}{\Sigma x P} = l$, for the length of the equivalent pendulum when the surface of the cylinder is supposed to be the centre of suspension; and it follows from the well known properties of the centre of gravity, that $\Sigma x P$ the sum of the product of all the particles into their distances, is equal to Qd, the product of the whole weight Q into the distance of the centre of gravity from the point of suspension; and $\Sigma x^2 P = \Sigma x P\, l = d Q l$, so that the equivalent length for the rolling pendulum becomes $\frac{d l Q}{\Sigma (x+r) P} = \frac{d l Q}{\Sigma x P + \Sigma r P} = \frac{d l Q}{d Q + r Q} = \frac{l}{1+\frac{r}{d}} = l\left(1-\frac{r}{d}\right)$, r being supposed very small; which, for a simple pendulum, when $d=l$, becomes $l-r$, as it ought to do. We must however find the displacement of the centre of suspension which is capable of producing an equal alteration in the length of the equivalent pendulum; and for this purpose we must have recourse to the theorem of HUYGENS, which may be easily deduced from the expression $\frac{\Sigma x x P}{dQ}$: for calling $x-d$, the distance of any particle of the body from its centre of gravity, y, we have $x^2 = (d+y)^2 = d^2 + 2dy + y^2$, and $\Sigma x^2 P = \Sigma d^2 P + 2d\Sigma y P + \Sigma y^2 P = d^2 Q + 0 + \Sigma y^2 P$, the integral of $\Sigma y P$, the product of the distance of each particle into its distance from the common centre of gravity always vanishing: consequently $l = \frac{\Sigma y y P + d^2 Q}{dQ} = \frac{\Sigma y y P}{dQ} + d$, and $l - d = \frac{\Sigma y y P}{dQ}$; which is HUYGENS's theorem: the constant quantity $\frac{\Sigma y y P}{Q}$ being equal

to $dl - d^2$. If now we suppose d to be increased by the small quantity s, the reciprocal, instead of $l - d$, will become $\frac{dl - dd}{d+s} = \frac{l-d}{1+\frac{s}{d}} = (l-d)\left(1-\frac{s}{d}\right) = l - d - l\frac{s}{d} + s$, to which adding $d + s$, we have $l - l\frac{s}{d} + 2s$, the increase of the length being $\frac{2d-l}{d}s$; and making this equal to $-\frac{l}{d}r$, we have $s = \frac{-lr}{2d-l}$: and when the pendulum is inverted, substituting $l - d$ for d, the expression becomes $\frac{-lr}{2l-2d-l} = \frac{lr}{2d-l}$, which, added to the former negative value of the same quantity, must always destroy it: so that the length of the equivalent pendulum will be truly measured by the simple distance of the surfaces of the cylinders, as M. Laplace has demonstrated.

There is however another correction, of which it becomes necessary to determine the value, when a very sharp edge is used for the axis of motion, as in the pendulum which you have employed: since it appears very possible, that in this case the temporary compression of the edge may produce a sensible elongation of the pendulum. But it will be found, by calculating the magnitude of this change, that when the edge is not extremely short, and when its bearing is perfectly equable, this correction may be safely neglected.

Supposing a to be the distance from the edge, in the plane bisecting its angle, at which the thickness is such, that the weight of the modulus of elasticity corresponding to the section shall become equal to the weight of the pendulum, the elasticity at any other distance x from the edge will be measured by x, while the weight is represented by a; so that the elementary increment x' will be reduced by the pressure of the weight to $\frac{x}{a+x}x'$, and the element of the compression

will be $\frac{a}{a+x}x'$, and its fluxion $\frac{a}{a+x}dx$, of which the fluent is $a\,\mathrm{HL}\frac{a+x}{a}$. Now the height of the modulus of elasticity of steel is ten million feet, (Lect. Nat. Phil. II. p. 509) and the weight of a bar, an inch square, and of this height, would be about 30 millions of pounds; so that if the weight be 10 pounds, and the line of bearing an inch long, the thickness at the distance a must be one three millionth of an inch; and supposing the angle a right one, a must be $\frac{1}{4244000}$; and making $x=1$, we have the whole compression of the edge within the depth of an inch $\frac{1}{4244000}\,\mathrm{HL}\,4244001$; and this logarithm being 15.26, the correction becomes equal to the 360 thousandth of an inch. If the bearing were one tenth of an inch only, the compression for both the opposite edges would become $\frac{1}{18000}$, supposing that they retained their elasticity, and underwent no permanent alteration of form. In fact, however, the edge must be considered as a portion of a minute cylinder, which will be still less compressible than an angle contained by planes; and the happy property, demonstrated by M. LAPLACE, will prevent any sensible inaccuracy from this cause, however blunt the edges may be, supposing that the steel is of uniform hardness in both.

Believe me, my dear Sir, very sincerely yours,

THOMAS YOUNG.

Welbeck Street, 5th Jan. 1818.

P. S. It is easy to show that the determination of the length of the pendulum, by means of a weight sliding on a rod or bar, which is the method that I have proposed as the most convenient for obtaining a correct standard, is equally independent of the magnitude of the cylinder employed. The

reduced inertia $\Sigma x^2 P$ here consists of two portions: for the rod we may take the equivalent expression dlQ, which we may call axy, a being the weight of the bar (Q), x the distance (d) of the centre of gravity, and y the equivalent length (l): for the ball we must employ the formula $\Sigma x^2 P = \Sigma y^2 P + d^2 Q$, and call $\Sigma y^2 P$, u, and $d^2 Q$, bz^2, b being the weight of the ball, and z the distance of its centre of gravity from the point of suspension: and in the same manner the force $\Sigma(x+r)P = (d+r)Q$ must be composed of the two portions $a(x+r)$ and $b(z+r)$, so that the equivalent length becomes $\frac{axy+u+bzz}{a(x+r)+b(z+r)} = \frac{x^2 + \frac{axy+u}{b}}{z + \frac{ax+ar+br}{b}}$; which we may call $\frac{xz+v}{x+w} = t$.

The experiment being then performed in four different positions of the weight, at the distances d', d'', and d''', so that the second value of z may be $z - d' = z'$, the third $z - d' = z''$, and the fourth $z - d''' = z'''$, we must observe the times of vibration, and deduce from them the comparative lengths of the equivalent pendulum, t, $n't$, $n''t$, and $n'''t$: and hence the value of z, of v, and of t may be obtained, without determining w, and of course without employing the quantity r.

First, $\frac{z^2+v}{z+w} = t$, $\frac{z'^2+v}{z'+w} = n't$, $\frac{z''^2+v}{z''+w} = n''t$, $\frac{z'''^2+v}{z'''+w} = n'''t$.

II, $z + w = \frac{z^2+v}{t}$, $z' + w = \frac{z'^2+v}{n't}$, $z'' + w = \frac{z''^2+v}{n''t}$, $z''' + w = \frac{z'''^2+v}{n'''t}$.

III, $z - z' = d'$; $z - z'' = d''$; $z - z''' = d'''$.

IV, $d' = \frac{z^2+v}{t} - \frac{z'^2+v}{n't}$; $d'' = \frac{z^2+v}{t} - \frac{z''^2+v}{n''t}$; $d''' = \frac{z^2+v}{t} - \frac{z'''^2+v}{n'''t}$.

V, $t = \frac{x^2+v}{d} - \frac{x''^2+v}{d'} = \frac{x^2+v}{d^2} - \frac{x''^2+v}{d'^2} = \frac{x^2+v}{d^2} - \frac{x'''^2+v}{d'^2}$.

VI. by comparing the first of these equations successively with the second and third, and bringing the terms containing v to the same side, we obtain

$$v = \left(\frac{x^2}{d^2} - \frac{x''^2}{d'^2} - \frac{x^2}{d^2} + \frac{x'''^2}{d'^2}\right) : \left(\frac{1}{d^2} - \frac{1}{d'^2} - \frac{1}{d^2} + \frac{1}{d'^2}\right) = \left(\frac{x^2}{d^2} - \frac{x''^2}{d'^2} - \frac{x^2}{d^2} + \frac{x'''^2}{d'^2}\right) : \left(\frac{1}{d^2} - \frac{1}{d'^2} - \frac{1}{d^2} + \frac{1}{d'^2}\right).$$

This equation contains only the squares of the values of x with known coefficients; and if we substitute $x - d'$, $x - d''$, and $x - d'''$ for x', x'', and x''', respectively, we shall obtain an equation in the form $ex^2 + fx = g$, whence $x = \pm\sqrt{(g + \frac{1}{4}f^2)} - \frac{1}{2}f$.

T. Y.

Fig. 1.

Fig. 2.

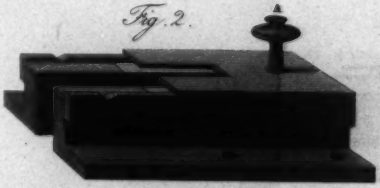


Fig. 5.

Mary F. Baker del.

Fig. 3.

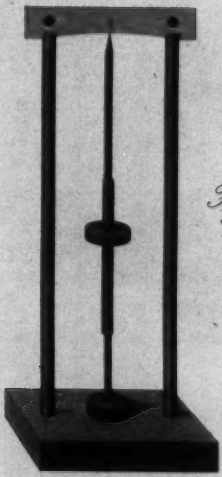
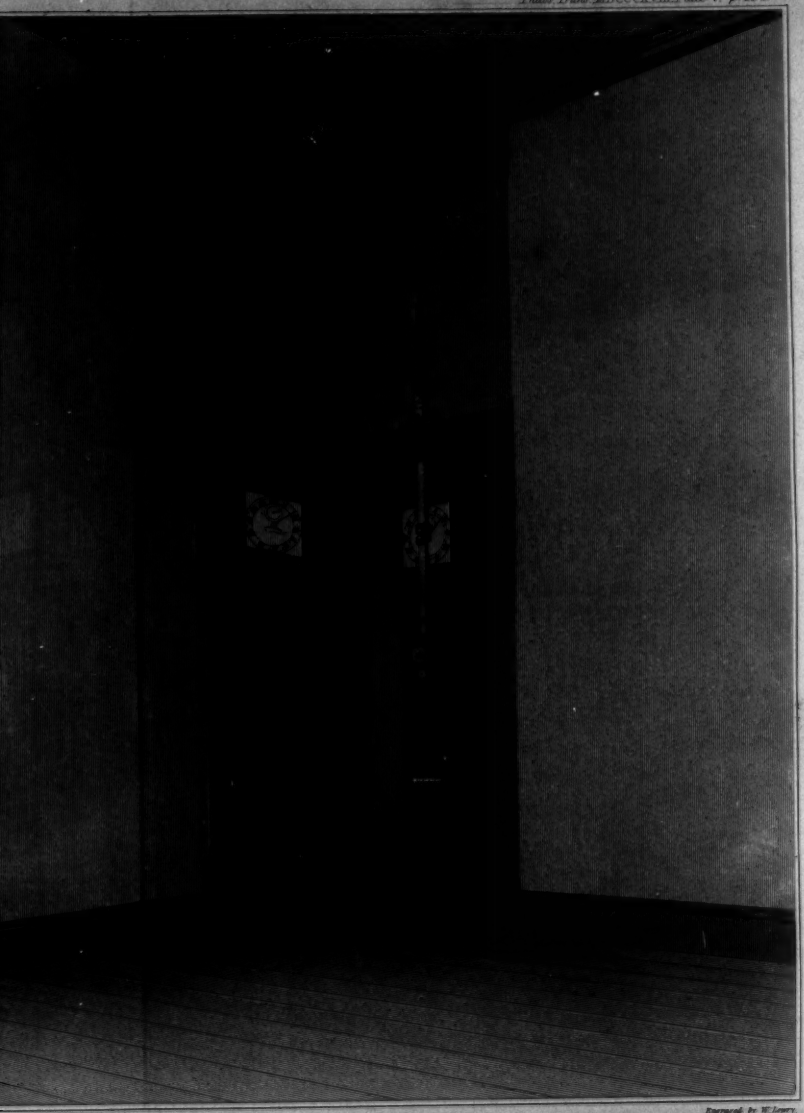


Fig. 4.

Fig. 5.

Caroline Brooke del.



Engraved by W. Lowry.

V. *On the length of the French Mètre estimated in parts of the English standard.* By *Captain* Henry Kater, *F. R. S.*

Read February 5, 1818.

One of the objects of the committee of the Royal Society appointed for the purpose of determining the length of the seconds pendulum, being the comparison of the French mètre with the British standard measure, two mètres were procured from Paris for that purpose, the one made in the usual manner and called the *mètre à bouts*, and the other a bar of platina on which the length of the mètre is shown by two very fine lines; this is named the *mètre à traits*.

The width of the *mètre à bouts* is one inch, and its thickness 0,8 of an inch. On one side the word "Metre" is engraved, and on the other "Fortin à Paris." The terminating planes are supposed to be perfectly parallel, and the distance between them is the length of the mètre.

The *mètre à traits* is the same width as the *mètre à bouts*, but only a quarter of an inch thick. The lines expressing the length of the mètre are so fine that one of them is scarcely perceptible even with the assistance of a microscope, unless the light be very favourable. The situation of the lines may however be discovered by two strong black dots made with a graver at the extremities of each, and a fine line crosses them at right angles to indicate the parts from which the measurements are to be taken.

This mètre previous to being brought from Paris, was

compared with a standard mètre by M. ARAGO, with all that care and ability which he is so well known to possess, and which so delicate an operation requires. The result was, that the distance between the lines was found to be less than a mètre by $\frac{17}{1000}$ of a millimètre or ,00069 of an inch.

The same micrometer microscopes were used in the comparisons which I am about to detail, as have been already described in my account of experiments on the length of the pendulum, in the Philosophical Transactions of the present year, and as the length of the mètre is nearly 39,4 inches, I was enabled to refer it to the same divisions of Sir GEORGE SHUCKBURGH's scale as I had employed in the measurement of the pendulum.

I commenced with the *mètre à traits*. It was placed in contact with the standard scale, their surfaces being in the same plane. An excellent thermometer was laid upon the scale, and a piece of thick leather was placed upon its bulb in order to prevent its being affected by heat from the person of the observer.

The whole was suffered to remain in this state for two or three days, after which the following observations were made at various times, the microscopes being brought alternately over the mètre and the scale. The value of each division of the micrometer is $\frac{1}{33363}$ of an inch.*

* For the manner in which this value was obtained, see page 51 of the preceding paper.

Comparison of the *mètre à traits*.

Temperature.	Reading of the microm. at 39,4 of the scale.	Reading of the microm. at the mètre.	Divisions to be deducted from 39,4 inches.	Distance in inches between the lines designating the mètre.	Correction for temperature in decimals of an inch.	Distance in inches between the lines designating the mètre, the mètre being at 32°, and the scale at 62°.
60,0	85,0	644,5	559,5	39,37606	,00604	39,37002
60,7	75,5	639,0	563,5	39,37588	,00589	39,36999
61,7	69,2	634,0	564,8	39,37583	,00568	39,37015
62,0	65,0	630,5	565,5	39,37580	,00562	39,37018
62,4	61,0	629,0	568,0	39,37569	,00554	39,37015
62,3	58,7	629,5	570,8	39,37557	,00556	39,37001
62,2	58,0	628,0	570,0	39,37560	,00558	39,37002
62,2	59,0	625,0	566,0	39,37577	,00558	39,37019
62,1	59,0	625,5	566,5	39,37575	,00560	39,37015
58,8	90,0	638,0	548,0	39,37654	,00629	39,37025
59,0	83,0	637,0	554,0	39,37629	,00625	39,37004
59,0	82,0	636,0	554,0	39,37629	,00625	39,37004
59,2	83,2	632,0	548,8	39,37651	,00621	39,37030
59,1	81,0	632,0	551,0	39,37642	,00623	39,37019
					Mean	39,37012
				The distance between the lines designating the mètre was found by M. ARAGO to be too little by a quantity = ,00069 of an inch which add - - -		+,00069
				The distance from zero to 39,4 of Sir G. SHUCKBURGH's scale is too short compared with the mean of its divisions by ,00005 of an inch which subtract* - - -		—,00005
				Hence the length of the mètre in inches of Sir G. SHUCKBURGH's scale is -		39,37076

The comparison of the *mètre à bouts*, presented considerable difficulties, which I conceive it would be of little use to detail, as the necessity of comparisons of this kind is of very rare occurrence; I shall therefore proceed to describe the method which was at last found successful.

* See page 54.

Four rectangular pieces of brass were prepared precisely similar to those described in the account of experiments on the pendulum in the Philosophical Transactions before referred to. These were marked C, *c*, D and *d*. The perfectly plane rectangular edges of the pieces C and *c*, being placed in contact, and kept thus by means of a spring, the distance of the fine lines drawn on their surfaces, parallel and very near to the rectangular edges, was found to be 500.5 divisions of the micrometer, and the pieces D and *d* being placed in like manner in contact, the distance of the lines on their surfaces estimated in the same divisions was 456.7.

The *mètre à bouts* being placed by the side of the brass scale and in contact with it, the pieces D and *d* were applied to its extremities, the surfaces of the brass pieces being a little below the surface of the mètre in order to preclude any error which might have arisen from the edges of the mètre projecting beyond its terminating planes. Each of the brass pieces was supported in this position upon a piece of lead of a sufficient thickness, and kept in close contact with the end of the mètre by means of a slight spring bearing against a pin driven perpendicularly into the lead.

In order to ensure a perfect contact between each brass piece and the terminating plane of the mètre, a flat ruler of brass was laid upon the surface of the mètre so as to project beyond its extremity, and the end of the lead was elevated or depressed so that the line of light seen between the piece of brass and the ruler, the eye being level with the surface of the brass, appeared to be equal in every part, when it was inferred that the surfaces of the mètre and of the piece of

brass were parallel, and consequently that their rectangular ends were perfectly in contact.

The distance between the lines on D and *d*, was now taken by the microscopes, and transferred to the scale in the manner before described; and when a sufficient number of comparisons had thus been made, the pieces D and *d*, were exchanged for those marked C and *c*, and the observations repeated with every precaution to ensure an accurate result, especially with respect to temperature.

The under surface of the mètre was then placed uppermost, and the apparatus being arranged as before, the same process was pursued as that which has just been described. The results are contained in the following tables.

Comparison of the *mètre à bouts*.

The pieces D and *d*, applied. Distance from D to *d*, 456,7 divisions. The word MÈTRE above.

Temperature.	Reading of the microm. at 39,4 of the scale.	Reading of the microm. at the brass pieces.	Difference.	Divisions to be deducted from 39,4 inches.	Length of the Mètre.	Corr. for temperature in decimals of an inch.	Length of the *Mètre*, the mètre being at 32 and the scale at 62°.
59,7	9,5	91,5	82,0	538,7	39,37694	,00610	39,37083
54,8	38,0	97,2	59,2	515,9	39,37792	,00713	39,37079
55,0	39,0	98,0	59,0	515,7	39,37793	,00709	39,37084
55,1	36,2	95,0	58,8	515,5	39,37794	,00707	39,37087
55,2	36,0	95,0	59,0	515,7	39,37793	,00705	39,37088
						Mean	39,37084

The pieces C and *c*, applied. Distance from C to *c*, 500,5 divs. The word MÈTRE above.

5,6	30,7	47,5	16,8	517,3	39,37786	,00696	39,37090
55,7	30,0	47,3	17,3	517,8	39,37784	,00694	39,37090
55,9	30,2	47,5	17,3	517,8	39,37784	,00690	39,37094
56,2	24,5	45,0	20,5	521,0	39,37770	,00684	39,37086
56,3	23,0	44,7	21,7	522,2	39,37765	,00681	39,37084
						Mean	39,37089

The pieces C and *c*, applied. Distance from C to *c*, 500,5 divisions. The word FORTIN above.

Temperature.	Reading of the microm. at 39,4 of the scale.	Reading of the microm. at the brass pieces.	Difference.	Divisions to be deducted from 39,4 inches.	Length of the Mètre.	Corr. for temperature in decimals of an inch.	Length of the Mètre, the mètre being at 32°, and the scale at 62°.
56,8	15,0	37,5	22,5	523,0	39,37762	,00671	39,37091
56,7	15,7	40,0	24,3	524,8	39,37754	,00673	39,37081
56,8	14,7	40,5	25,8	526,3	39,37747	,00671	39,37076
56,8	15,5	40,0	24,5	525,0	39,37753	,00671	39,37082
56,8	15,5	40,0	24,5	525,0	39,37753	,00671	39,37082
						Mean	39,37082

The pieces D and *d*, applied. Distance from D to *d*, 456,7 divisions. The word FORTIN above.

55,0	35,3	93,0	57,7	514,4	39,37798	,00709	39,37089
55,2	36,5	95,0	58,5	515,2	39,37795	,00705	39,37090
55,2	36,0	93,0	57,0	513,7	39,37801	,00705	39,37096
56,0	26,5	89,0	62,5	519,2	39,37778	,00688	39,37090
56,0	23,5	87,0	63,5	520,2	39,37773	,00688	39,37085
						Mean	39,37090

Summary of the preceding comparisons.

The word "MÈTRE" above -	{ D and *d*, 39,37084 / C and *c*, 39,37089 }	39,37087
"FORTIN"	{ C and *c*, 39,37082 / D and *d*, 39,37090 }	39,37086
	Mean.	39,37086
Subtract for error in division of the scale -		—,00005
Length of the *Mètre à bouts* in inches of Sir G. SHUCKBURGH's scale - - - -		39,37081

The following is the manner in which the correction for temperature was obtained. The expansion of platina according to the experiments of BORDA and others, is ,00000476

parts of its length for one degree of FAHRENHEIT, and as this is the expansion used by the French in adjusting the length of their mètre, it must be employed on the present occasion. The mètre being taken at 32°, the expansion for the difference between this and the temperature of measurement, must be subtracted from the apparent length of the mètre. The English standard temperature is 62°, therefore if the temperature of measurement be under this, the expansion of the scale for such difference of temperature must be deducted from the length of the mètre before obtained. These two corrections are combined in the column entitled "correction for temperature." Sir G. SHUCKBURGH's standard scale is of cast brass, and as I could not conveniently determine its actual expansion with that degree of accuracy that would have satisfied me, I have taken for it, the mean result of two experiments made on plate brass, which gave me an expansion of ,0000101 parts of its length for one degree of FAHRENHEIT. The mean of most of the experiments made on the expansion of brass gives ,0000104, and had I employed this last number instead of my own, the difference in the length of the mètre would have been utterly inconsiderable.

Supposing then both mètres to be of equal authority, we have for the length of the *mètre à traits* 39,37076, and for that of the *mètre à bouts* 39,37081 inches; the mean of which, 39,37079, may be taken for the length of the mètre in inches of Sir G. SHUCKBURGH's standard scale when each is brought to its proper temperature.*

* The length of the mètre compared with BIRD's parliamentary standard is 39,37062 inches.

London, November 1817.

VI. *A few facts relative to the colouring matters of some vegetables.* By James Smithson, *Esq. F. R. S.*

Read December 18, 1817.

I began, a great many years ago, some researches on the colouring matters of vegetables. From the enquiry being to be prosecuted only at a particular season of the year, the great delicacy of the experiments, and the great care required in them, and consequently the trouble with which they were attended, very little was done. I have now no idea of pursuing the subject.

In destroying lately the memorandums of the experiments which had been made, a few scattered facts were met with which seemed deserving of being preserved. They are here offered, in hopes that they will induce some other person to give extension to an investigation interesting to chemistry and to the art of dying.

Turnsol.

M. Fourcroy has advanced, somewhere, that turnsol is essentially of a red colour; and that it is made blue by an addition of carbonate of soda to it; and he says that he has extracted this salt from the turnsol of the shops.

If turnsol contained carbonate of soda, its infusions should precipitate earths and metals from acids.

I did not find an infusion of turnsol in water to have the least effect on solutions of muriate of lime, nitrate of lead, muriate of platina, or oxalate of potash.

Its tinctures, or infusions, consequently, contain neither any alkali, nor any lime; nor probably any acid, either loose or combined. This is unfavourable to the opinion of urine being employed in the preparation of turnsol.

I put a little sulphuric acid into a tincture of turnsol, then added chalk, and heated; and the blue colour was restored. It appears, therefore, that the natural colour of turnsol is not red, but blue, since it is such when neither disengaged acid or alkali is present.

No addition of chalk brought the cold liquor back to a blue colour; the carbonic acid absorbed by it, during the effervescence of the carbonate of lime, being sufficient to keep it red.

Some turnsol was put into distilled vinegar. An effervescence arose; and after some time the acid was become neutralized. On examining the mixture with a glass, there were seen, at the bottom of the vessel, a multitude of grains like sand. It was found on trial that these grains were carbonate of lime; probably of slightly calcined Carrara marble.

When turnsol is treated with water till this no longer acquires any colour whatever, the remaining insoluble matter is nearly as blue as at first.

Acids made this blue insoluble matter red, but did not extract any red tincture.

Carbonate of soda did not affect it.

If the vegetable part of this blue residuum is burned away, or it is washed off with water, a portion of smalt is obtained.

On exhaling, on a water bath, a tincture of turnsol, the colouring matter is left in a dry state.

This matter heated in a platina spoon over a candle, tumefied considerably, as much as starch does, became black and smoked, but did not readily inflame, nor did it burn away till the blowpipe was applied. It then burned pretty readily, leaving a large quantity of a white saline matter. This saline matter saturated by nitric acid afforded crystals of nitrate of potash, and some minute crystals like hydrous sulphate of lime.

Is this potash merely that portion of this matter which exists in all vegetable substances? or is the colouring matter of turnsol a compound, analogous to ulmin, of a vegetable principle and potash? Its low combustibility gives some sanction to this idea.

Of the colouring matter of the violet.

The violet is well known to be coloured by a blue matter which acids change to red; and alkalies and their carbonates first to green and then to yellow.

This same matter is the tinging principle of many other vegetables: of some, in its blue state; of others, made red by an acid.

If the petals of the red rose are triturated with a little water and carbonate of lime, a blue liquor is obtained. Alkalis, and soluble carbonates of alkalis, render this blue liquor green; and acids restore its red colour.

The colouring matter of the violet exists in the petals of red clover, the red tips of those of the common daisy of the fields, of the blue hyacinth, the holly hock, lavender, in the inner leaves of the artichoke, and in numerous other flowers. It likewise, made red by an acid, colours the skin of several

plumbs, and, I think, of the scarlet geranium, and of the pomegranate tree.

The red cabbage, and the rind of the long radish are also coloured by this principle. It is remarkable that these, on being merely bruised, become blue; and give a blue infusion with water. It is probable that the reddening acid in these cases is the carbonic; and which, on the rupture of the vessels which enclose it, escapes into the atmosphere.

Of sugar-loaf paper.

This paper has been employed by BERGMAN as a chemical instrument. I am ignorant of what it is coloured with.

Sulphuric, muriatic, nitric, phosphoric, and oxalic acids make it red. Tartaric and citric acids, made rather yellow spots than red ones. Distilled vinegar, and acid of amber, had no effect on it.

Carbonate of soda and caustic potash did not alter the blue colour of this paper.

Water boiled on this paper acquired a vinous red colour; carbonate of lime put into this red liquor, did not affect its colour: nor did carbonate of soda or caustic potash change it to blue or green.

Cold dilute sulphuric acid extracted a strong yellow tincture from this boiled paper: carbonate of lime put to this yellow tincture made it blue; but on filtering, the liquor which passed was of a dirty greenish colour; and sulphuric acid did not make it red: a blue matter was left on the filter, which was not made red by acetous acid; but was so by sulphuric.

After this treatment the paper remained brown; seemingly such as it was before being dyed blue.

It should seem that there are at least two colouring matters in this paper; one red, which is extricable from it by water; the other blue, which requires the agency of an acid to extract it.

Its insolubility in water, and low degree of sensibility to acids, distinguish the blue matter from turnsol; to which its not being affected by alkalis otherwise much approximate it. Its easy solubility in dilute sulphuric acid, and being reddened by it and several other acids, show it not to be indigo.

Of the black mulberry.

The expressed juice of this fruit is of a fine red colour.

Caustic potash made it green, which gradually became yellow.

Carbonate of soda did not make it green, but only blue.

Carbonate of ammonia changed it to a vinous red, rather than to blue; and this redness increased on standing.

Caustic ammonia made it bluer than its carbonate; but, on standing, the mixture became of the same vinous red.

The mulberry juice mixed with carbonate of lime became purple. On filtering, a red liquor passed; and the carbonate of lime left on the filter was blue. An addition of whitening to the red filtered liquor did not alter its colour; nor did this second portion of whitening become blue. Heating did not affect the red colour of this liquor; so that it was not owing to carbonic acid, disengaged from the carbonate of lime. Caustic potash instantly made this red liquor a fine green, and gradually yellow.

Sulphuric acid rendered all the above mixtures florid red. It is remarkable that the mixtures with ammonia, and carbonate of ammonia, which were become quite vinous red by standing, were made a perfect blue by the sulphuric acid before they were reddened by it. It would hence seem that the red colour, caused by these alkalis, was owing to an excess of them; and that in a less quantity they would have produced a blue.

The filter, into which the mixture of mulberry juice and chalk had been thrown, was become tinged blue. Water did not remove this colour. Sulphuric acid made this paper florid red. Caustic potash did not alter its blue colour; but put on the places made red by sulphuric acid, it restored the blue colour, but did not produce green.

Future experiments must decide whether this blue matter is the same as that of turnsol; or as the blue matter which the experiments above have indicated in sugar-loaf paper.

The juices of many other fruits, as black cherries, red currants, the skin of the berries of the buckthorn, elder berries, privet berries, &c., seem to be made only blue by mild fixed alkalis, but green by caustic. Puzzling anomalies, however, occasionally present themselves, which seem to show a near relation between the several blue colouring matters of vegetables, and their easy transition into one another.

The corn poppy.

The petals of the common red poppy of the fields rubbed on paper stain it of a reddish purple colour.

Solution of carbonate of soda put to this stain occasioned but little change in it.

Caustic potash made it green.

Caustic ammonia seemed not to have more effect on it than carbonate of soda.

Some poppy petals being bruised in a mixture of water and marine acid, formed a florid red solution: a superabundance of chalk added to this red liquor, did not make it blue; but turned it to a dark red colour exactly like port wine.

Some poppy petals bruised in a weak solution of carbonate of soda, and the mixture filtered, the liquor which came through was not at all blue, but of a dark red colour like port wine. Caustic potash made this red liquor green, which finally became yellow.

Some dried poppy petals of the shops, gave a strong obscure vinous tincture to cold water. This red tincture heated with whitening, did not alter to blue, but preserved its red colour.

These very imperfect experiments may perhaps suggest the idea, that the colouring matter of this flower is the same as the red colouring matter of the mulberry.

Of sap green.

The inspissated juice of the ripe, or semi-ripe, berries of the buckthorn, constitute the pigment called sap green; by the French, vert de vessie. This species of green matter is entirely different from the common green matter of vegetables.

It is soluble in water.

Carbonate of soda and caustic potash changed the solution of sap green to yellow. Paper tinged by sap green is a sensible test of alkalis.

Sulphuric, nitric, and marine acid, made it red. Carbonate of lime added to a reddened solution, restored the green colour, which therefore appears to be the proper colour of the substance.

The green colour, which the last infusions of galls present, appears to be different, both from the usual green of vegetables, and from sap green.

Some animal greens.

A green puceron, or aphis, being crushed on white paper, emitted a green juice, which was immediately made yellow by carbonate of potash (wrongly called sub-carbonate.)

There are small gnats of a green colour: crushed on paper, they make a green stain, which is permanent. Neither muriatic acid nor carbonate of soda altered this green colour. It is consequently of a different nature from the foregoing.

VII. *Account of experiments made on the strength of materials. By* George Rennie, *jun. Esq. In a Letter to* Thomas Young, *M. D. For. Sec. R. S.*

Read February 19th, 1818.

DEAR SIR, *London, June* 3, 1817.

In presenting you the result of the following experiments, I trust I shall not be considered as deviating from my subject, in taking a cursory view of the labours of others. The knowledge of the properties of bodies which come more immediately under our observation, is so instrumental to the progress of science, that any approximation to it deserves our serious attention. The passage over a deep and rapid river, the construction of a great and noble edifice, or the combination of a more complicated piece of mechanism, are arts so peculiarly subservient to the application of these principles, that we cannot be said to proceed with safety and certainty, until we have assigned their just limits. The vague results, on which the more refined calculations of many of the most eminent writers are founded, have given rise to such a multiplicity of contradictory conclusions, that it is difficult to choose, or distinguish, the real from that which is merely specious. The connections are frequently so distant, that little reliance can be placed on them. The Royal Society appears to have instituted, at an early period, some experiments on this subject, but they have recorded little to aid us. Emerson, in his Mechanics, has laid down a number of rules,

and approximations. Professor ROBISON in his excellent treatise in the Encyclopædia Britannica; BANKS on the power of machines; Dr. ANDERSON of Glasgow; Colonel BEAUFOY, &c. are those, amongst our countrymen, who have given the result of their experiments on wood, and iron. The subject, however, appears to have excited considerable attention on the continent. A theory was published in the year 1638, by GALILEO, on the resistance of solids, and subsequently, by many other philosophers. But however plausible these investigations appeared, they were more theoretical than practical, as will be seen in the sequel. It is only by deriving a theory from careful and well directed experiments, that practical results can be obtained. It would be useless to enumerate the labours of those philosophers, who in following, or varying from the steps of GALILEO, have merely tended to obscure a subject respecting which they had no data to proceed upon. It is sufficient to enumerate the names of those who, in conjunction with our own countrymen, have added their labours to the little knowledge we possess. The experiments of BUFFON, recorded in the Annals of the Academy of Sciences at Paris, in the years 1740 and 1741, were on a scale sufficiently large to justify every conclusion, had he not omitted to ascertain the direct and absolute strength of the timber employed. It however appeared from his experiments, that the strength of the ligneous fibre is nearly in proportion to the specific gravity. MUSCHENBROECK, whose accuracy (it is said) entitled him to confidence, made a number of experiments on wood and iron, which by being tried on various specimens of the same materials, afforded a mean result considerably higher than other previous authorities. Experiments have

also been made by MARIOTTE, VARIGNON, PERRONET, RAMUS, RONDELET, GAUTHEY, NAVIER, AUBRY and TEDIER DE NORBECK, as also at the Ecole Polytechnique, under the direction of M. PRONY. With such authorities before us, it might be deemed presumption in me, to offer you a communication on a subject which had been previously treated of by so many able men. But whoever has had occasion to investigate the principles upon which any edifice is constructed, where the combination of its parts are more the result of uncertain rules than sound principle, will soon find how scanty is our knowledge on a subject so highly important. The desire of obtaining some approximation, which could only be accomplished by repeated trials on the substances themselves, induced me to undertake the following experiments; for which purpose I ordered an apparatus to be prepared, of which the two annexed plates [Plates VI. and VII.] are representations.

Description of the Apparatus.

A bar of the best English iron, about 10 feet long, was selected and formed into a lever (whose fulcrum is denoted by *f*). The hole was accurately bored, and the pin turned, which suffered it to move freely. The standard (A) was firmly secured by the nut (*c*) to a strong bed plate of cast iron, made firm to the ground. The lever was accurately divided in its lower edge, which was made straight in a line with the fulcrum. A point, or division (D), was selected, at 5 inches from the fulcrum, at which place was let in a piece of hardened steel. The lever was balanced by the balance weight (E), and in this state it was ready for operation. But in order to keep it as level as possible, a hole was drilled

through a projection on the bed plate, large enough to admit a stout bolt easily through it, which again was prevented from turning in the hole by means of a tongue (t) fitting into a corresponding groove in the hole. So that, in order to preserve the level, we had only to move the nut to elevate, or depress the bolt, according to the size of the specimen. But as an inequality of pressure would still arise from the nature of the apparatus, the body to be examined was placed between two pieces of steel, the pressure being communicated through the medium of two pieces of thick leather above, and below the steel pieces, by which means a more equal contact of surfaces was attained. The scale was hung on a loop of iron, touching the lever in an edge only. I at first used a rope for the balance weight, which indicated a friction of four pounds, but a chain diminished the friction one half. Every moveable centre was well oiled. Of the resistances opposed to the simple strains which may disturb the quiescent state of a body, the principal are the repulsive force, whereby it resists compression, and the force of cohesion, whereby it resists extension. On the former, with the exception of the experiments of GAUTHEY and RONDELET, on stones, and a few others, on soft substances, there is scarcely any thing on record. In the memoir of M. LAGRANGE, on the force of springs, published in the year 1760, the moment of elasticity is represented by a constant quantity, without indicating the relation of this value to the size of the spring: but, in the memoir of the year 1770, on the forms of columns, where he considers a body whose dimensions and thickness are variable, he makes the moment of elasticity proportional to

the fourth power of the radius, in observing the relations of theory and practice to accord with each other. This was admitted by EULER in his memoir of 1780, in his elaborate investigation of the forms of columns. Mr. COULOMB had however shown before that time, how inapplicable all these calculations were to columns under common circumstances; and you, Sir, have repeated the observation in your lectures on natural philosophy. The results of experiments have also been equally discordant; since it is deduced from those of REYNOLDS, that the power required to crush a cubic quarter of an inch of cast iron, is 448000lbs. avoirdupoise, or 200 tons; whereas by the average of thirteen experiments made by me on cubes of the same size, the amount never exceeded 10392.58lbs, not quite five tons.* This may be seen by referring to the tables. There were four kinds of iron used, viz. 1st. iron taken from the centre of a large block, whose crystals were similar in appearance and magnitude to those evinced in the fracture of what is usually termed gun metal. 2ndly. Iron taken from a small casting, close grained, and of a dull grey colour. 3rdly. Iron cast horizontally in bars of $\frac{1}{8}$th inches square, 8 inches long. 4thly. Iron cast vertically, same size as last. These castings were reduced equally on every side to $\frac{1}{4}$ of an inch square: thus removing the hard external coat usually surrounding metal castings. They were all subjected to a gauge. The bars were then presumed to be

* It is probable that Mr. REYNOLDS made his experiments on metal cast at the furnace of Maidley Wood, which is of a very strong and superior quality; but this circumstance can have been but of little importance compared to the great disproportion of the results.

tolerably uniform. The weights used were of the best kind that could be procured, and as the experiment advanced, smaller weights were used.

Experiments on cast iron in cubes of $\frac{1}{8}$ of an inch, &c.

Iron taken from the block whose specific gravity was 7,088.

Averages.		lbs. avoirdupoise.
1439.66	$\frac{1}{8} \times \frac{1}{8}$	1454
	$\frac{1}{8} \times \frac{7}{8}$	1416
	$\frac{1}{8} \times \frac{1}{8}$	1449

On specimens of different lengths. Specific gravity of iron 6,977.

2116	$\frac{1}{8} \times \frac{2}{8}$	1922
	$\frac{1}{8} \times \frac{3}{8}$	2310
1758.5	$\frac{1}{8} \times \frac{3}{8}$ slipped with 1863lbs. filed flat, and crushed with	2363
	$\frac{1}{8} \times \frac{4}{8}$ ditto, 1,495, ditto	2005
	$\frac{1}{8} \times \frac{5}{8}$ ditto,	1407
	$\frac{1}{8} \times \frac{6}{8}$ ditto,	1743
	$\frac{1}{8} \times \frac{7}{8}$ ditto,	1594
	$\frac{1}{8} \times \frac{8}{8}$ ditto,	1439

April 23*d*, 1817. *Experiments on cubes of $\frac{1}{4}$ of an inch taken from the block.*

9778.5	$\frac{1}{4} \times \frac{1}{4}$	10561
	$\frac{1}{4} \times \frac{1}{4}$	9596
	$\frac{1}{4} \times \frac{1}{4}$	9917
	$\frac{1}{4} \times \frac{1}{4}$	9020

Castings, Horizontal. Specific gravity 7.113.

Averages.		lbs. avoirdupoise.
10114	$\frac{1}{4} \times \frac{1}{4}$ - - - -	10432
	$\frac{1}{4} \times \frac{1}{4}$ - - - -	10720
	$\frac{1}{4} \times \frac{1}{4}$ - - - -	10605
	$\frac{1}{4} \times \frac{1}{4}$ - - - -	8699

Vertical castings. Specific gravity 7.074.

11138675	$\frac{1}{4} \times \frac{1}{4}$ bottom of vertical bar -	12665
	$\frac{1}{4} \times \frac{1}{4}$ - - - -	10950
	$\frac{1}{4} \times \frac{1}{4}$ - - - -	11088
	$\frac{1}{4} \times \frac{1}{4}$ - - - -	9844
	$\frac{1}{4} \times \frac{1}{4}$ full size. Scale broke with 10294; tried again - -	11006

A prism, having a logarithmic curve for its limits, resembling a column; it was $\frac{1}{4}$ of an inch diameter by one inch long, broke with - - - 6954

April 28*th. Trials on prisms of different lengths.*

9414.5	$\frac{1}{4} \times \frac{1}{2}$ horizontal - - -	9455
	$\frac{1}{4} \times \frac{1}{2}$ ditto - - - -	9374
	$\frac{1}{4} \times \frac{1}{2}$ ditto, bad trial, 9006 lbs.	
9982.5	$\frac{1}{4} \times \frac{1}{2}$ vertical - - -	9938
	$\frac{1}{4} \times \frac{1}{2}$ ditto - - - -	10027

April 29th. **Horizontal Castings.**

Averages.		lbs. avoirdupoise.
$\frac{1}{4} \times \frac{3}{8}$	- - - -	9006
$\frac{1}{4} \times \frac{5}{8}$	- - -	8845
$\frac{1}{4} \times \frac{6}{8}$	- - - -	8362
$\frac{1}{4} \times \frac{7}{8}$	- - -	6430
$\frac{1}{4} \times \frac{1}{8}$	or one inch long - -	6821

Vertical castings.

$\frac{1}{4} \times \frac{3}{8}$	- - - -	9328
$\frac{1}{4} \times \frac{5}{8}$	- - -	8385
$\frac{1}{4} \times \frac{6}{8}$	a small defect in the specimen -	7896
$\frac{1}{4} \times \frac{7}{8}$	- - - -	7018
$\frac{1}{4} \times \frac{1}{8}$	or one inch - - -	6430

Experiments on different metals.

$\frac{1}{4} \times \frac{1}{4}$	cast copper, crumbled with	-	-	7318
$\frac{1}{4} \times \frac{1}{4}$	fine yellow brass reduced $\frac{1}{10}$ with	3218.	$\frac{1}{2}$ with	10304
$\frac{1}{4} \times \frac{1}{4}$	wrought copper, -	$\frac{1}{16}$	3427. $\frac{1}{8}$	6440
$\frac{1}{4} \times \frac{1}{4}$	cast tin, -	$\frac{1}{16}$	552. $\frac{1}{2}$	966
$\frac{1}{4} \times \frac{1}{4}$	cast lead, - - -		$\frac{1}{2}$	483

The anomaly between the three first experiments on $\frac{1}{8}$ cubes, and the two second of a different length, can only be accounted for, on the difficulty of reducing such small specimens to an equality. The experiments on $\frac{1}{8}$ inch prisms of different lengths give no ratio. The experiments on $\frac{1}{4}$ inch cubes, taking an average of the three first in each, give a proportion between them and the three on $\frac{1}{8}$ cubes,

as 1: 6.096 in the block castings
as 1: 7.352 in the horizontal ditto
as 1: 8.035 in the vertical ditto

in several cases the proportion is as the cubes.

The vertical cube castings are stronger than the horizontal cube castings.

The prisms usually assumed a curve similar to a curve of the third order, previous to breaking.

The experiments on the different metals, give no satisfactory results. The difficulty consists in assigning a value to the different degrees of diminution. When compressed beyond a certain thickness, the resistance becomes enormous.

Experiments on the suspension of bars.

The lever was used as in the former case, but the metals were held by nippers, as indicated in the drawing No. 2. They were made of wrought iron, and their ends adapted to receive the bars, which, by being tapered at both extremities, and increasing in diameter from the actual section (if I may so express it), and the jaws of the nippers being confined by a hoop, confined both. The bars, which were six inches long, and ¼ square, were thus fairly and firmly grasped.

April 30*th*, 1817.

		lbs.	
No. 45	¼ inch, cast iron bar, horizontal -	1166	1193·5
46	¼ do. do. vertical -	1218	
47	¼ do. cast steel previously tilted -	8391	
48	¼ do. blister steel, reduced per hammer	8922	
49	¼ do. shear steel, do. do. -	7977	
50	¼ do. Swedish iron, do. do. -	4504	
51	¼ do. English iron, do. do. -	3492	
52	¼ do. hard gun metal, mean of two trials	2273	
53	¼ do. wrought copper reduced per hammer - -	2112	
54	¼ do. cast copper - -	1192	

No. 55 ¼ do. fine yellow brass - 1128
56 ¼ do. cast tin - - - 296
57 ¼ do. cast lead - - 114

Remarks on the last experiments.

The ratio of the repulsion of the horizontal cast cubes to the cohesion of horizontal cast bars, is 8.65 : 1.

The ratio of the vertical cast cubes to the cohesion of the vertical cast bars, is as 9.14 : 1.

The average of the bars, compared with the cube, No. 16, is as 10.611 : 1.

The other metals decrease in strength, from cast steel to cast lead.

The stretching of all the wrought bars indicated heat.

The fracture of the cast bars was attended with very little diminution of section, scarcely sensible.

The experiment made by M. PRONY, (which asserts, that by making a slight incision with the file, the resistance is diminished one half) was tried on a ¼ inch bar of English iron; the result was 2920lbs., not a sixth part less.

This single experiment, however, does not sufficiently disprove the authority of that able philosopher, for an incision is but a vague term. The incision I made might be about the 40th part of an inch.

Experiments on the twist of ¼ inch bars.

To effect the operation of twisting off a bar, another apparatus was prepared: it consisted of a wrought iron lever two feet long, having an arched head about ⅛ of a circle, of 4 feet diameter, of which the lever represented the radius, the

centre round which it moved had a square hole made to receive the end of the bar to be twisted. The lever was balanced as before, and a scale hung on the arched head; the other end of the bar being fixed in a square hole in a piece of iron, and that again in a vice. The undermentioned weights represent the quantity of weight put into the scale.

May 30th, 1817.

On twists close to the bearing, cast horizontal.

No.		lbs.	oz.	
58	¼ in bars, twisted as under with	10	14	in the scale.
59	¼ do. bad casting	8	4	
60	¼ do.	10	11	
	average	9	15	

Cast vertical.

61	¼	10	8	
62	¼	10	13	
63	¼	10	11	
		10	10	

On different metals.

64	Cast steel	17	9	
65	Shear steel	17	1	
66	Blister steel	16	11	
67	English iron, wrought	10	2	
68	Swedish iron, wrought	9	8	
69	Hard gun metal	5	0	
70	Fine yellow brass	4	11	
71	Copper, cast	4	5	
72	Tin	1	7	
73	Lead	1	0	

On twists of different lengths.

No.		Weight in scale.
	Horizontal.	
74	$\frac{1}{4}$ by $\frac{1}{4}$ long	7 3
75	$\frac{1}{4}$ by $\frac{3}{4}$ do.	8 1
76	$\frac{1}{4}$ by 1 inch do.	8 8
	Vertical.	
77	$\frac{1}{4}$ by $\frac{1}{4}$ do.	10 1
78	$\frac{1}{4}$ by $\frac{1}{4}$ do.	8 9
79	$\frac{1}{4}$ by 1 inch do.	8 5

Horizontal twists at 6 from the bearing.

80	$\frac{1}{4}$ by 6 inches long	10 9
81	$\frac{1}{4}$ by do. do.	9 4
82	$\frac{1}{4}$ by do. do.	9 7

Twists of $\frac{1}{2}$ inch square bars, cast horizontally.

		qrs.	lbs.	oz.	
83	$\frac{1}{2}$ close to the bearing	3	9	12	end of the bar hard.
84	$\frac{1}{2}$ do.	2	18	0	middle of the bar.
85	$\frac{1}{2}$ at 10 inches from bearing, lever in the middle	1	24	0	

On twists of different materials.

These experiments were made close to the bearing, and the weights were accumulated in the scale until the substances were wrenched asunder.

86	Cast steel	19 9
87	Shear steel	17 1
88	Blister steel	16 11
89	English iron, No. 1.	10 2

No.		Weight in scale.
90	Swedish iron	9 8
91	Hard gun metal	5 0
92	Fine yellow brass	4 11
93	Copper	4 5
94	Tin	1 7
95	Lead	1 0

Remarks.

Here the strength of the vertical bars still predominates.

The average of the two taken conjointly, and compared with a similar case of $\frac{1}{4}$ inch bars, gives the ratio as the cubes, as was anticipated.

In the horizontal castings of different lengths, the balance is in favour of the increased lengths; but in the vertical castings, it is the reverse. In neither is there any apparent ratio. In the horizontal castings at 6 inches from the bearing, there is a visible increase, but not so great as when close to the bearing.

June 4th, 1817. *Miscellaneous experiments on the crush of one cubic inch.*

No.		lbs. avoirdupoise.
96	Elm	1284
97	American pine	1606
98	White deal	1928
99	English oak, mean of two trials	3860
100	Ditto, of 5 inches long, slipped with	2572
101	Ditto, of 4 inches do.	5147
102	A prism of Portland stone 2 inches long	805
103	Ditto, statuary marble	3216
104	Craig Leith	8688

In the following experiments on stones, the pressure was communicated through a kind of pyramid, the base of which rested on the hide leather, and that, on the stone. The lever pressed upon the apex of the pyramid. Cubes of one and a half inch.

		specific gravity.	lbs. avoirdu.
105	Chalk		1127
106	Brick of a pale red colour	2.085	1265
107	Roe stone, Gloucestershire		1449
108	Red brick, mean of two trials	2.168	1817
109	Yellow face baked Hammersmith paviors 3 times		2254
110	Burnt do. mean of two trials		3243
111	Stourbridge or fire brick		3864
112	Derby grit, a red friable sand stone	2.316	7070
113	Ditto, from another quarry	2.428	9776
114	Killaly white freestone, not stratified	2.423	10264
115	Portland	2.428	10284
116	Craig Leith, white freestone	2.452	12346

June 5th, 6th, and 7th, 1817.

117	Yorkshire paving with the strata	2.507	12856
118	Ditto, do. against the strata	2.507	12856
119	White statuary marble not veined	2.760	13632
120	Bramley Fall sand stone, near Leeds, with strata	2.506	13632
121	Ditto, against the strata	2.506	13632
122	Cornish granite	2.662	14302
123	Dundee sand stone or Brescia, two kinds	2.530	14918
124	A two inch cube of Portland	2.423	14918

No.		specific gravity.	lbs. avoir.
125	Craig Leith with the strata -	2.452	15560
126	Devonshire red marble, variegated		16712
127	Compact limestone - -	2.584	17354
128	Peterhead granite hard close grained		18636
129	Black compact limestone, Limerick	2.598	19924
130	Purbeck - -	2.599	20610
131	Black Brabant marble -	2.697	20742
132	Very hard freestone -	2.528	21254
133	White Italian veined marble -	2.726	21788
134	Aberdeen granite, blue kind -	2.625	24556

N. B. The specific gravities were taken with a delicate balance, made by CREIGHTON of Glasgow, all with the exception of two specimens which were by accident omitted.

Remarks.

In observing the results presented by the preceding table, it will be seen that little dependence can be placed on the specific gravities of stones, so far as regards their repulsive powers, although the increase is certainly in favour of their specific gravities. But there would appear to be some undefined law in the connection of bodies, with which the specific gravity has little to do. Thus, statuary marble has a specific gravity above Aberdeen granite, yet a repulsive power not much above half the latter. Again, hardness is not altogether a characteristic of strength, inasmuch as the limestones, which yield readily to the scratch, have nevertheless a repulsive power approaching to granite itself.

It is a curious fact in the rupture of amorphous stones, that pyramids are formed, having for their base the upper side of the cube next the lever, the action of which displaces the

sides of the cubes, precisely as if a wedge had operated between them. I have preserved a number of the specimens, the sides of which, if continued, might cut the cubes in the direction of their diagonals.

Experiments made on the transverse strain of cast bars, the ends loose. June 8th, 1817.

		Weight of the bars. lbs.	oz.	Dist. of bearings ft.		lbs. avoir.
135	Bar of 1 inch square -	10	6	3	0	897
136	Do. of 1 inch, do. -	9	8	2	8	1086
137	half the above bar -			1	4	2810
138	Bar of 1 inch square, through the diagonal -	2	8	2	8	851
139	Half the above bar -		-	1	4	1587
140	Bar of 2 in. deep, by $\frac{1}{2}$ inch thick	9	5	2	8	2183
141	Half the above bar -		-	1	4	4508
142	Bar 3 in. deep, by $\frac{1}{2}$ inch thick	9	15	2	8	3588
143	Half the bar - -		-	1	4	6854
144	Bar 4 inches, by $\frac{1}{2}$ inch thick -	9	7	2	8	3979
145	Equilateral triangles with the angle up and down.					
146	Edge or angle up -	9	11	2	8	1487
147	——— angle down -	9	7	2	8	840
148	Half the first bar -		-	1	4	3059
149	Half the second bar -		-	1	4	1656
150	A feather-edged or ⊥ bar was cast whose dimensions were					
151	2 inches deep by 2 wide	10	0 edge up	2	8	3105
152	Half of ditto					

N. B. All these bars contained the same area, though differently distributed as to their forms.

Experiments made on the bar of 4 inches deep by $\frac{1}{4}$ inch thick, by giving it different forms, the bearings at 2 feet 8 inches, as before.

		lbs.	lbs.
153	Bar formed into a semi-ellipse, weighed	7	4000
154	Ditto, parabolic on its lower edge	-	3860
	Ditto, of 4 inches deep by $\frac{1}{4}$ inches thick	-	3979

Experiments on the transverse strain of bars, one end made fast, the weight being suspended at the other, at 2 feet 8 inches from the bearing.

155	An inch square bar bore	- -	280
156	A bar 2 inches deep, by $\frac{1}{2}$ an inch thick	-	589
157	An inch bar, the ends made fast	- -	1173

The paradoxical experiment of EMERSON was tried, which states that by cutting off a portion of an equilateral triangle (see page 114 of EMERSON'S Mechanics) the bar is stronger than before, that is, a part stronger than the whole. The ends were loose at 2 feet 8 inches apart as before. The edge from which the part was intercepted, was lowermost, the weight was applied on the base above, it broke with 1129 lbs., whereas, in the other case it bore only 840lbs.

Remarks on the transverse strain.

BANKS makes his bar from the cupola, when placed on bearings 3 feet asunder, and the ends loose, to bear 864lbs

Now all my bars were cast from the cupola, the difference was therefore - - - 33lbs.

I adopted a space of 2 feet 8 inches asunder, as being more convenient for my apparatus. The strength of the different bars, all cases being the same, approaches nearly to the

theory, which makes the comparative values as the breadths multiplied into the squares of the depths. The halves of the bars were tried, merely to keep up the analogy. The bar of 4 inches deep, however, falls short of theory by 365 lbs. It is evident we cannot extend the system of deepening the bar much farther, nor does the theory exactly maintain in the case of the equilateral triangle by - - 248lbs. The diagonal position of the square bar, is actually worse than when laid on its side, contrary to many assertions.

The same quantity of metal in the feather edged bar, was not so strong as in the 4 inch bar.

The semi-elliptical bar, exceeded the 4 inch bar, although taken out of it. The parabolic bar came near it.

The bar made fast at both ends, I suspect must have yielded, although the ends were made fast by iron straps. The experiments from EMERSON, on solids of different forms might be made; but the time and trouble these experiments have already cost, have compelled me to relinquish farther pursuits for the present. If, however, in the absence of better, they are worthy of the indulgence of the Royal Society, it will not only be a consolation to me that my labours merit their attention, but a farther inducement to prosecute the investigation of useful facts, which, even in the present advanced state of knowledge, will yet admit of addition.

I am, with much respect,

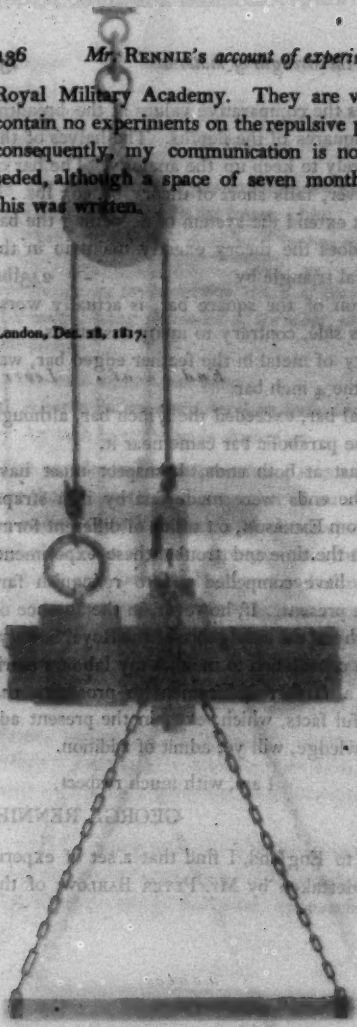
GEORGE RENNIE.

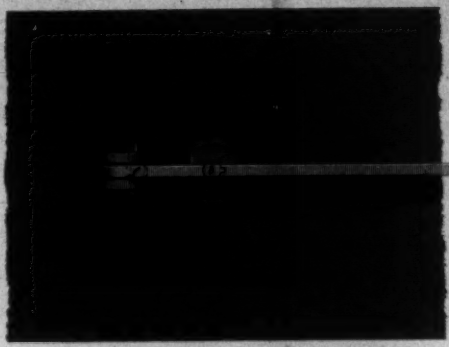
Since my return to England, I find that a set of experiments have been undertaken by Mr. PETER BARLOW, of the

Royal Military Academy. They are very interesting, but contain no experiments on the repulsive power of bodies, and consequently, my communication is not altogether superseded, although a space of seven months has elapsed since this was written.

G. R.

London, Dec. 28, 1817.





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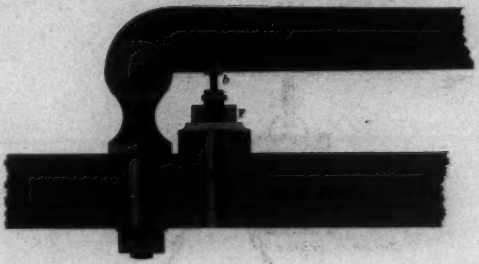
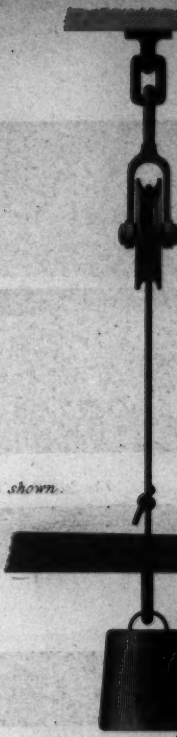
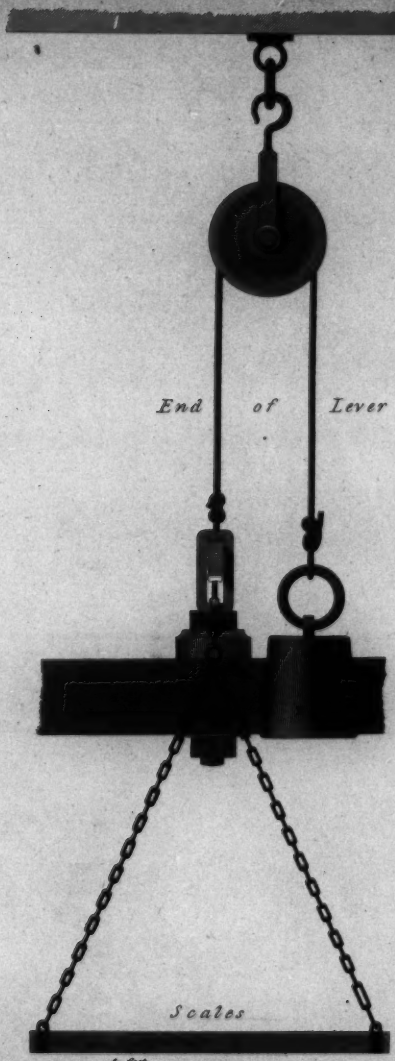




Balance weight

Scale of one ℔ a

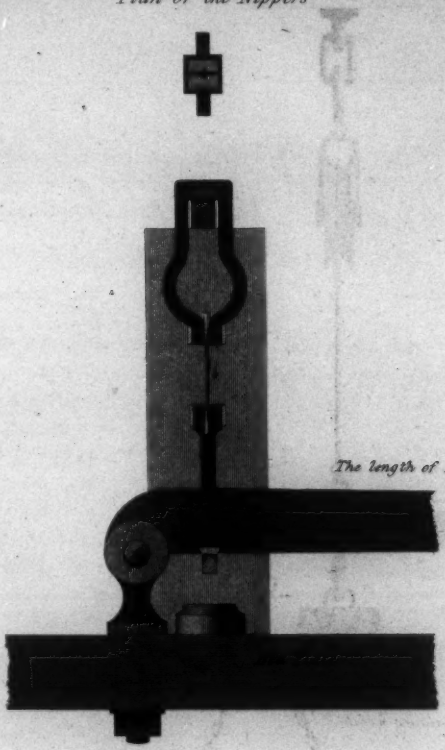
G. Rennie, del.



2 3 4 Feet

t a half Inches to the Foot

Plan of the Nippers



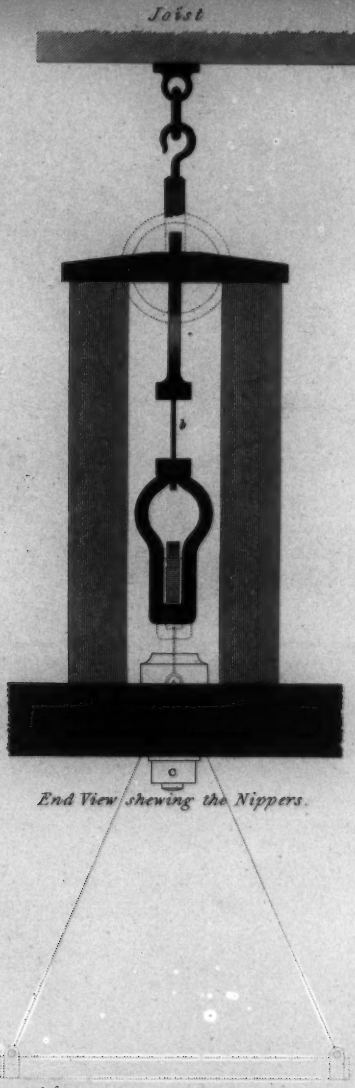
The length of Lever is not shewn

Joist

Balance weight

Scale of one & a ha

G. Rennie, del.



J.C. Barrow del.

VIII. *On the office of the heart wood of trees.* *By* T. A. Knight, *Esq. F. R. S. In a Letter addressed to the Rt. Hon. Sir* Joseph Banks, *Bart. G. C. B. P. R. S.*

Read February 5, 1818.

MY DEAR SIR,

TREES of every species, that afford timber, live many years before any portion of their alburnum becomes converted into heart wood; and vegetation proceeds with as much vigour previously to the existence of that substance, as subsequently. In the oak it is rarely seen till the seedling tree becomes nearly twenty years old; when it is readily distinguishable from the alburnum by a deeper colour, higher specific gravity, and greater hardness. The tubes also, which extend through the tree longitudinally, and are always open in the alburnum, so as freely to permit the passage of air or water, are closed in the heart wood; and the cellular substance of it has appeared, in every experiment that has come under my observation, to be incapable of conveying the ascending fluid. It does not therefore appear to execute any very important office in the vegetable economy; farther than that it obviously gives, as I have remarked in a former communication, much additional strength to the stem and branches, when these, particularly the latter, become more subject to receive injury, both from the influence of winds and gravitation, on account of the increased distance of their foliage from the points of suspension. Its mode of operation in this case

appears, however, to be purely mechanical, and not to be in any degree dependent upon the vital power of the tree; and some writers on vegetable physiology have regarded it as a wholly lifeless substance. This opinion I have always rejected, though I was unable to adduce any decisive evidence in opposition to it; but I have now reason to believe that the heart wood becomes, during winter, in common with the alburnum and bark, a reservoir of the organizable matter which the tree expends in germination in the spring; and that the fluid sap passes abundantly into it laterally, though it does not ascend through it.

I had long previously been perfectly satisfied that every species of tree, and perennial plant, contains within itself, during winter, all the organizable matter which it employs in the formation of its first foliage, and succulent shoots, in the spring; and that it is owing to the presence or absence of such reservoir, that the lives of plants become annual, biennial, and perennial. The annual plant exhausts itself wholly in feeding its flowers and seeds: it forms no reservoir, and consequently perishes. Its vital powers are not expended, for detached parts of the same plant, and obviously possessing the same life, become perennial, if planted in such season that they cannot exhaust themselves by the production of flowers before winter. A biennial plant (the common turnip affords a good and familiar example) fills its reservoir in one season, and wholly expends it in the following, when it consequently dies, like the annual plant. In the tree, as in the biennial plant, a part of the reserved sap descends early in the spring to form new roots, whilst another portion ascends to feed its buds; but the tree also discharges laterally a large

quantity of organizable matter to generate a new layer of bark over the whole surface of its alburnum, and it thus adapts its organization to its more permanent state of existence.

The alburnum and bark of trees of some species did not appear to me to be capable of containing in winter so much organizable matter as I saw thus expended in particular cases; and I found the thickness of both these substances to be subject to great variation in trees of the same species, and of equally vigorous growth. I also observed that the heart wood of the oak, and of other trees, always contained much uncombined soluble matter; and that its external layers annually acquired greater specific weight and density, owing to the reception of new matter, which must, I conceived, have been conveyed into it in a fluid state. I was therefore led to ascertain what quantity of fluid is retained by the heart wood of the oak, and some other species of trees in winter; and this enquiry appeared interesting, because it has long been well known, that very considerable chemical changes take place in the sap of trees in the spring; and that the alburnum of trees, which are not felled before that period, is brittle, and subject to decay very rapidly. The heart wood of winter-felled oak trees was also formerly very generally supposed to be much more valuable than that of trees of the same species when felled in the spring; and I expected to obtain some evidence in support of this opinion: for if the same fluid pervade alike the heart wood and alburnum of trees, it does not appear probable that the qualities of the one will remain unchanged, when those of the other sustain so much injury.

Pieces of the external, or last formed layer of the alburnum, and of the heart wood, were taken from the trunk (near the ground) of an oak of forty years old, and of very vigorous growth, in the first week of December. These were slowly dried in a temperature not exceeding at any time a hundred and twenty degrees; under which circumstances 1000 parts of the alburnum lost 469 parts, and an equal quantity of the heart wood 500 parts, or precisely half its weight. Upon the 24th of April, similar pieces of the alburnum and heart wood were taken from the same tree, and dried in the same manner; when 1000 parts of the alburnum lost in drying 532 parts, and an equal quantity of the heart wood 507 parts.

The alburnum and heart wood of a poplar tree of about 80 years old were subjected to similar management in December; when 1000 grains of the alburnous substance lost 535 grains, and an equal quantity of the heart wood 626 grains. On the first of March, 1000 grains of alburnum being taken from a tree of similar age and habit with the preceding, lost in drying 557 grains; and the same quantity of heart wood 684 grains. Not only the cellular substance of the heart wood, but the tubes also, which are usually quite empty in the alburnum, were perfectly filled; so that the heart wood of the poplar exhibited nearly the semi-transparency of horn; and in subsequent experience I have found that it contains nearly the same quantity of water in every part of the year.

This abundant fluid in the heart wood was some years ago first observed by M. COULOMB, when felling poplar trees in

the spring. But he conceived it to be merely the fluid, which was ascending from the earth, at that period; and he concluded from subsequent observation and experiment, in the same season of the year, and in the summer, that the sap of trees chiefly passes up in the vicinity of their medulla, through their heart wood.

M. COULOMB's statements are, I entertain no doubt, perfectly correct; but the inferences which he, and other continental naturalists have drawn from the facts which he has stated, are, I suspect, erroneous. For I have stated in a former communication, that when I intersected the alburnum of an oak tree in winter, no symptoms of life appeared above such intersection in the ensuing spring. A similar experiment being repeated in the end of June, appeared instantly to intercept the whole of the ascending current, and the leaves of a tree, of which the heart wood remained entire, faded more rapidly than those of another tree of the same species, which was felled at the same period, and lay upon the ground. In the spring of 1816 also, upon the 1st of March, the season of M. COULOMB's experiments, I intersected the alburnum of a poplar tree, 1000 parts of which I have stated to have contained 684 parts water, and an equal quantity of its alburnum 557 parts: yet this tree, notwithstanding the immense quantity of water (probably little less than a ton) which it contained, exhibited very feeble signs of life in the following month, though the weather continued excessively wet; and before Midsummer it was perfectly lifeless. The elongated cellular, or (as it is usually called) the fibrous texture of the alburnum, through which the sap is now, I

believe, very generally supposed to ascend, appeared to have become impervious upon the conversion of alburnum into heart wood; whilst the lateral or convergent cellular processes remained open to permit the ingress of the moving fluid, without which the heart wood could not probably long retain life.

I must therefore reject the hypothesis which assumes the ascent of the sap through the heart wood, and must believe that the fluid, which affords the organizable matter that is annually deposited in the conversion of alburnum into heart wood, and which subsequently gives greater solidity and strength to that substance, is derived from the bark; and that it passes inwards during the latter part of the summer and autumn, through the convergent cellular (or medullary) processes, to return in part through the same passages when a new layer of bark is to be formed in the spring. Under such circumstances the operation of the heart wood, when it exists in large quantity comparatively with the bark and foliage, as in sound pollard oak trees, must tend to check and diminish, rather than to promote, growth; and amongst trees of this description I have often been able readily to select such as were sound by the slowness of the growth of their branches comparatively with those of other trees of apparently the same age, which were become hollow.

Whether the heart wood of oak trees, which are deprived of their bark in the spring, and suffered to remain standing till the following winter, recover the whole, or a part of the good qualities which it loses (or is supposed to lose) in the spring, is a very interesting question. A few experiments

which I have made, lead me to think it probable, that both the alburnum and heart wood become improved in quality under such circumstances; but I am not at present in possession of such facts as can enable me to give any decisive opinion.

I am, my dear Sir, with great regard,

sincerely yours,

T. A. KNIGHT.

Downton, Dec. 24th, 1817.

IX. *On circulating functions, and on the integration of a class of equations of finite differences into which they enter as coefficients.* *By* John F. W. Herschel, *Esq. F. R. S.*

Read February 19, 1818.

(1). So much has been written on the subject of recurring series, and the equations of differences from which they arise, that we can now expect little more to be added to their theory. This is not the case with the class of series, and their corresponding equations I propose to consider in the following pages, which bear a great analogy to the other, and include them as a particular case: I mean, series in which the same relation between a certain number of successive terms recurs periodically; the terms so related being separated by others connected by relations similar in their general analytical form, but modified by a variation in the constant or variable coefficients which enter into the equations expressing them. Such series have, I believe, never yet been considered as a class: particular cases have very frequently occurred in the course of analytical investigations, and have then been treated by peculiar considerations of such a description as to give a very uninviting air to their theory, but no general view has hitherto been taken of their nature, and no uniform train of analytical artifices been exposed by whose aid they may be subjected to the same modes of treatment as those of the ordinary kind.

(2). Let us imagine a series of quantities

$$u_0, u_1, u_2, u_3, \text{ \&c.}$$

produced from one another by the following regular law,

$$u_0 = u_0, u_1 = au_0, u_2 = bu_1, u_3 = au_2, u_4 = bu_3, \text{ \&c.}$$

It is evident then that, were the coefficients a, b, equal, the series would be a recurring one of the simplest kind, viz. a geometric progression, and might be represented by a single equation of differences of the first order

$$u_{x+1} = au_x$$

If this, however, be not the case, the series will consist of two distinct geometric progressions, the terms of which alternate with one another, thus

$$u = u_0 \qquad u_2 = abu_0 \qquad u_4 = a^2b^2u_0 \qquad \text{\&c.}$$
$$u_1 = au_0 \qquad u_3 = a^2bu_0 \qquad u_5 = a^3b^2u_0, \text{ \&c.}$$

It would seem then that no single equation of differences of the first order could comprehend all the terms of this series, so as to pass uninterruptedly from one to the other; and were this really the case, the method which has hitherto been always followed, of actually resolving it into the distinct series of which it consists, and instituting a separate process for the odd and the even values of x, so as to get the two equations of differences

$$u_{2x+1} = a \cdot u_{2x}, \text{ and } u_{2x+2} = b \cdot u_{2x+1}$$

would be the only course we could pursue. That this, however, is not the case, at least in the instance before us, is evident, if we consider that both these equations are included in the equation of the second order,

$$u_{x+2} = abu_x$$

the first integral of which will be an equation of the first order comprehending the whole extent of the series, provided the constant be properly determined by the equation $u_1 = au_0$.

At all events however several inconveniences embarrass this method. It is entirely at variance with the uniformity which ought to reign throughout all analytical operations, thus to descend into arithmetical details in the outset of symbolic investigation, and to vary our processes according to the numerical form of the quantities concerned. If we would avoid this by having recourse to an equation of a higher order including all the separate cases, we are required either to seize at once, by an undirected effort of intellect, on the relation which connects the terms periodically equidistant, or to go through some preparatory process to discover it, which for the most part will be found very troublesome. Moreover, it demands the actual formation of certain terms to determine the constants, which are not (as will hereafter appear) necessary. It is true that in the very simple case I have just stated, these inconveniences, though really existing, are not felt. If we take one a little more complicated, they will speedily form a prominent part of the difficulty. Suppose the law of formation of the terms of a series were, for instance, as follows:

$$u_2 = au_1 + \alpha u_0,\ u_3 = bu_2 + \beta u_1,\ u_4 = cu_3 + \alpha u_2,$$

$$u_5 = au_4 + \beta u_3,\ u_6 = bu_5 + \alpha u_4,\ u_7 = cu_6 + \beta u_5,\ \&c.$$

it would be necessary to divide the whole investigation into six cases, and to integrate six several equations of differences,

$$u_{6x+2} = au_{6x+1} + \alpha u_{6x},\ u_{6x+3} = bu_{6x+2} + \beta u_{6x+1},\ \&c.$$

and after all, the *general term* of the series would not be obtained, but merely the several general terms of six other series which, interlaced, as it were, one with the other, form the series in question; which is in fact much the same way

of proceeding as it would be to consider the series of natural numbers as consisting of several other arithmetical progressions such as

1 . . 4 . . 7 . . 10 . . 13 . &c.
2 . . 5 . . 8 . . 11 . . 14 . &c.
3 . . 6 . . 9 . . 12 . . 15, &c.

the general terms of which are respectively $3x-2$, $3x-1$, $3x$, united into one, the general term of which is x.

It is interesting then to discover some analytical artifice which shall obviate these inconveniences, by comprehending the whole extent of these and similar series in one single equation, whose order shall be no higher than is absolutely indispensible; which shall require no preparatory investigation to obtain it; nor the actual calculation of any superfluous terms for the determination of the constants in its integral; and, finally, whose integration shall lead to an expression in functions of the index x, such that the substitution of the natural progression of numbers in succession for x shall produce all the terms of the series in their order. Such an artifice, or train of artifices, I shall now proceed to explain. They turn upon a theorem familiar to every algebraist, but which does not seem to have been yet applied to all the uses of which it is susceptible.

(3). Let us then represent by $S_x^{(n)}$ the function

$$\frac{\alpha^x+\beta^x+\gamma^x+\&c.}{n}$$

α, β, γ, &c. being the several roots of the equation $x^n-1=0$. If we have occasion to denote other functions similarly composed of the roots of other equations $x^2-1=0$, $x^3-1=0$, &c. they will therefore be represented as follows:

$$S_x^{(p)} = \frac{\alpha^{px} + \beta^{px} + \&c.}{p}, \quad S_x^{(q)} = \frac{\alpha'^{qx} + \beta'^{qx} + \&c.}{q}, \&c.$$

but when only one quantity n is under consideration, we shall, for convenience, omit the superior index (n) and write the function thus

$$S_x = \frac{\alpha^x + \beta^x + \&c.}{n}$$

Let us also denote by $P_x^{(n)}$ the function

$$a_x \cdot S_x^{(n)} + b_x \cdot S_{x-1}^{(n)} + c_x \cdot S_{x-2}^{(n)} + \ldots\ldots k_x \cdot S_{x-n+1}^{(n)}$$

omitting, in like manner, the superior index (n) and writing it P_x when only one quantity n is considered. Here we suppose a_x, b_x, &c. to represent any given functions of x and constant quantities. The functions S_x and P_x are possessed then of the following properties.

(4). S_x is unity whenever x is a multiple of n: in all other cases, $S_x = o$. This is a well known property of the roots of unity. Hence, some one of the functions

$$S_x, S_{x-1}, S_{x-2}, \ldots\ldots S_{x-n+1}$$

is necessarily unity, the rest being all zero, though when the numerical form of x is not specified, it is undecided which that one may be. Hence too it follows, that when x is a multiple of n, P_x or

$$a_x \cdot S_x + b_x \cdot S_{x-1} + \ldots\ldots k_x \cdot S_{x-n+1}$$

reduces itself to a_x; when $x-1$ is a multiple of n, to b_x when $x-2$ is such a multiple, to c_x, and so on. Thus in all cases P_x will reduce itself to a single term, the form of which will be either $a_x, b_x, \ldots\ldots k_x$, in rotation, after which the same functions recur over again, for some one of the numbers

$x,\ x-1,\ x-2,\ \ldots\ldots\ x-n+1$

is necessarily a multiple of n.

If the coefficients a_x, b_x, &c. be all constant, or if

$$P_x = a \cdot S_x + b \cdot S_{x-1} + \ldots\ldots k \cdot S_{x-n+1}$$

and we give to x the several values 0, 1, 2, 3, 4, &c. to infinity, in succession, the first n values of P_x will be in their order

$$a, b, c, \ldots\ldots k$$

after which the same set of quantities will be reproduced in the same order by continuing the substitution, and so on to infinity. The function P_x may be called in this case a *circulating function*, and the same name (with less propriety however) may be extended to the case when the coefficients are variable. The system of coefficients $a, b, c, \ldots\ldots k$ may be called a period. Hence if the terms of a series be in rotation, $a, b, c, \ldots k, a, b,$ &c. the general term will be truly represented by P_x, or $aS_x + bS_{x-1} + \ldots\ldots kS_{x-n+1}$, and if they coincide in rotation with the values of n functions a_x, b_x, &c. thus:

$$a_0, b_1, c_2, \ldots k_{n-1}, a_n, b_{n+1},\ \&c.$$

the general term will be

$$a_x \cdot S_x + b_x \cdot S_{x-1} + \ldots\ldots k_x \cdot S_{x-n+1}$$

(5). Any function, however complicated, of $x, S_x, S_{x-1}, \ldots\ldots S_{x-n+1}$ is reducible to the form P_x.

Let $\varphi(x) = f\left\{x, S_x, S_{x-1}, \ldots\ldots S_{x-n+1}\right\}$

be the proposed function. If then x be of the form mn, or a multiple of n, we have $S_x = 1$, and the rest zero, therefore in this case

$$\varphi(x) = f\left\{x, 1, 0, 0, \ldots 0\right\}$$

If x be of the form $mn+1$, or if $x-1$ be a multiple of n, we have in like manner

$$\phi(x) = f\{x, 0, 1, 0, \ldots . 0\}$$

and so on. If then we take

$$a_x = f\{x, 1, 0, 0, \ldots 0\}$$
$$b_x = f\{x, 0, 1, 0, \ldots . 0\}$$
$$\ldots\ldots\ldots\ldots$$
$$k_x = f\{x, 0, 0, 0, \ldots 1\}$$

the whole series of values of $\phi(x)$ will coincide, term by term, to infinity, with the corresponding values of

$$P_x = a_x . S_x + b_x . S_{x-1} + \ldots . . k_x . S_{x-n+1}$$

and therefore $\phi(x) = P_x$, integer values of x only being considered.

For example,

$$a_x = a_x . S_x + a_x . S_{x-1} + \ldots . . a_x . S_{x-n+1};$$

$$\{a_x . S_x + b_x . S_{x-1} + \ldots k_x . S_{x-n+1}\} \times \{a'_x . S_x + b'_x . S_{x-1} + \ldots . k'_x . S_{x-n+1}\}$$
$$= a_x . a'_x . S_x + b_x . b'_x . S_{x-1} + \ldots . k_x . k'_x . S_{x-n+1};$$

$$\{a . S_x + b . S_{x-1} + \ldots k . S_{x-n+1}\}^m = a^m . S_x + b^m . S_{x-1} + \ldots k^m . S_{x-n+1}.$$

It should be remarked, that any number of the coefficients a_x, b_x, &c. may be zero, without infringing on the truth of this or the following propositions.

(6). Every symmetrical function of $S_x, S_{x-1}, \ldots . S_{x-n+1}$ is invariable.

Let $f\{\bar{x}, \bar{y}, \bar{z}, \text{&c.}\}$ denote a symmetrical function of any number of elements x, y, z, &c. (in the manner proposed

by Mr. BABBAGE in the Philosophical Transactions for 1816) and let us consider the symmetrical function

$$f\left\{\overline{S}_x, \overline{S}_{x-1}, \ldots \overline{S}_{x-n+1}\right\}$$

Then, by the nature of the roots of unity as demonstrated in most treatises on algebra, we have

$$S_x = S_{x-n} = S_{x-2n} = \&c.$$
$$S_{x-1} = S_{x-n+1} = S_{x-2n+1} = \&c. \text{ and so on.}$$

If then x be changed to $x+1$, the above function will become

$$f\left\{\overline{S}_{x-n+1}, \overline{S}_x, \ldots \ldots \overline{S}_{x-n+2}\right\}$$

that is, by the nature of symmetrical functions, (the order in which the elements are operated on making no difference in their form)

$$f\left\{\overline{S}_x, \overline{S}_{x-1}, \ldots \overline{S}_{x-n+2}, \overline{S}_{x-n+1}\right\}$$

the same as before. The function therefore remains unaltered, while x changes to $x+1$, and is therefore invariable. Hence we have in general

$$f\left\{\overline{S}_x, \overline{S}_{x-1}, \ldots \overline{S}_{x-n+1}\right\} = f\left\{\overline{1}, \overline{0}, \ldots \overline{0}\right\}$$

This proposition may be extended to any number of functions of the form $S_x^{(n)}$, $S_x^{(m)}$, &c. and to functions symmetrical in different senses relative to each set of them: thus we have

$$f\left\{\overline{S_x^{(n)}}, \overline{S_{x-1}^{(n)}}, \ldots \overline{S_{x-n+1}^{(n)}}, \overline{S_x^{(m)}}, \ldots \overline{S_{x-m+1}^{(m)}}, \overline{S_x^{(p)}}, \ldots \overline{S_{x-p+1}^{(p)}}, \&c.\right\}$$
$$= f\left\{\overline{1}, \overline{0}, \ldots \overline{0}, \overline{1}, \overline{0}, \overline{1}, \ldots \overline{0}, \&c.\right\}$$

the accents pointing out the different kinds of symmetry observed in the composition of the function relative to its several elements.

(7). If P_x denote any circulating function with constant coefficients, as

$$a S_x + b S_{x-1} + \ldots\ldots k S_{x-n+1}$$

Then, 1st, $P_x = P_{x-n} = P_{x-2n} =$ &c. and 2dly,

$$f\{P_x, P_{x-1}, \ldots\ldots P_{x-n+1}\} = f\{a, b, \ldots\ldots k\}$$

For in the expression of P_x write successively $x, x-1, x-2, \ldots\ldots x-n$ for x, and we have (since $S_{x-n} = S_x$, &c.)

$$\left.\begin{aligned} P_x &= aS_x + bS_{x-1} + \ldots\ldots kS_{x-n+1} \\ P_{x-1} &= aS_{x-1} + bS_{x-2} + \ldots\ldots kS_x \\ P_{x-2} &= aS_{x-2} + bS_{x-3} + \ldots\ldots kS_{x-1} \\ &\ldots\ldots\ldots\ldots\ldots\ldots \\ P_{x-n+1} &= aS_{x-n+1} + bS_x + \ldots\ldots kS_{x-n+2} \end{aligned}\right\}; (A)$$

and lastly $P_{x-n} = aS_x + bS_{x-1} + \ldots kS_{x-n+1} = P_x$.

Any function then, symmetrical relative to all the first members of the equations (A) will, by reason of the circulating form of their second members be also symmetrical relative to $S_x, S_{x-1}, \ldots\ldots S_{x-n+1}$, whence by the last proposition the truth of this becomes evident. For example

$$P_x . P_{x-1} \ldots . P_{x-n+1} = a . b \ldots . k.$$

$$P_x + P_{x-1} + \ldots P_{x-n+1} = a + b + \ldots . k.$$

(8). We are now prepared to proceed to the integration of circulating equations, and to determine by that means the general terms of such series as depend on them. The manner of reducing the law of a series of this kind into an equation will be rendered evident by a simple instance. Suppose this law to be as follows:*

* This example is selected as having actually occurred in an enquiry of another nature.

$u_2 = au_1 + u_0$, $u_3 = bu_2 + u_1$, $u_4 = au_3 + u_2$,
$u_5 = bu_4 + u_3$, &c.

The period of the coefficients of the second terms of all these equations being a, b, if we take S_x to represent the sum of the xth powers of the roots of $z^n - 1 = 0$, the circulating function $aS_x + bS_{x-1}$ will express the general term of the series $a, b, a, b,$ &c. and the equation

$$u_x = (aS_x + bS_{x-1})u_{x-1} + u_{x-2}$$

will coincide in succession with each of the given ones, by giving x every integer value from 2 to infinity. This then is the equation of the series, which it only remains to integrate. To this end assume

$$u_x = v_x \cdot \sqrt{aS_x + bS_{x-1}} = v_x \cdot \sqrt{P_x}$$

putting P_x for $aS_x + bS_{x-1}$. Then, by (7) we have $P_{x-2} = P_x$ and the equation becomes, by substitution,

$$v_x \cdot \sqrt{P_x} = P_x \cdot \sqrt{P_{x-1}} \cdot v_{x-1} + v_{x-2} \cdot \sqrt{P_x}$$

that is,

$$v_x = v_{x-1} \cdot \sqrt{\frac{P_x}{P_{x-1}}} + v_{x-2}$$

but, $\sqrt{P_x \cdot P_{x-1}}$ being a symmetrical function of P_x and P_{x-1} is by (7) invariable, and equal to $\sqrt{ab}$, whence we have

$$v_x = \sqrt{ab} \cdot v_{x-1} + v_{x-2}$$

an ordinary equation with constant coefficients, and consequently integrable by the usual methods. Similar considerations will enable us to arrive at the equations of all periodical series, and we shall therefore confine our attention to the equations alone, in the most extended form of which they are susceptible.

(9). *Given the circulating equation*

$$u_x + {}^1P_x . u_{x-1} + {}^2P_x . u_{x-2} + \ldots\ldots {}^mP_x . u_{x-m} = {}^{m+1}P_x$$

where ${}^1P_x$, ${}^2P_x$, &c. *are any circulating functions, with either constant or variable coefficients, the period of their circulation being the same in each, and equal to* n; *to integrate it*

Assume $u_x = {}^1A_x . S_x + {}^2A_x . S_{x-1} + \ldots\ldots {}^nA_x . S_{x-n+1}$

Then we have

$$u_{x-1} = {}^nA_{x-1} . S_x + {}^1A_{x-1} . S_{x-1} + \ldots\ldots {}^{n-1}A_{x-1} . S_{x-n+1}$$

$$u_{x-2} = {}^{n-1}A_{x-2} . S_x + {}^nA_{x-2} . S_{x-1} + \ldots\ldots {}^{n-2}A_{x-2} . S_{x-n+1}$$

and so on; and supposing

$${}^1P_x = {}^1a_x . S_x + {}^1b_x . S_{x-1} + \ldots\ldots {}^1k_x . S_{x-n+1}, \text{ &c.}$$

we shall have by (5),

$$u_{x-1} . {}^1P_x = {}^1a_x . {}^nA_{x-1} . S_x + {}^1b_x . {}^1A_{x-1} . S_{x-1} + \ldots\ldots$$
$$\ldots\ldots {}^1k_x . {}^{n-1}A_{x-1} . S_{x-n+1},$$

similarly,

$$u_{x-2} . {}^2P_x = {}^2a_x . {}^{n-1}A_{x-2} . S_x + {}^2b_x . {}^nA_{x-2} . S_{x-1} + \ldots\ldots$$
$$\ldots\ldots {}^2k_x . {}^{n-2}A_{x-2} . S_{x-n+1}, \text{ &c.} = \text{&c.}$$

The equation then will become by substitution

$$0 = S_x \left\{ {}^1A_x + {}^1a_x {}^nA_{x-1} + {}^2a_x {}^{n-1}A_{x-2} + \ldots\ldots \right.$$
$$\left. \ldots\ldots {}^ma_x . {}^{n-m+1}A_{x-m} - {}^{m+1}a_x \right\}$$
$$+ S_{x-1} \left\{ {}^2A_x + {}^1b_x . {}^1A_{x-1} + {}^2b_x {}^nA_{x-2} + \ldots\ldots \right.$$
$$\left. \ldots\ldots {}^mb_x . {}^{n-m+2}A_{x-m} - {}^{m+1}b_x \right\}$$
$$+ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$
$$+ S_{x-n+1} \left\{ {}^nA_x + {}^1k_x {}^{n-1}A_{x-1} + {}^2k_x {}^{n-2}A_{x-2} + \ldots\ldots \right.$$
$$\left. \ldots\ldots {}^mk_x . {}^{n-m}A_{x-m} - {}^{m+1}k_x \right\}; \text{ (B).}$$

n has here been supposed greater than m. If the contrary

be the case, when the progression ${}^{n}A_x, {}^{n-1}A_x, \ldots {}^{1}A_x$ is exhausted, instead of going on to write ${}^{-1}A_x, {}^{-2}A_x$, we must begin over again with ${}^{n}A_x$, &c. and so continue in regular rotation as often as may be necessary. Now to determine these functions, let each term be separately made to vanish, and a system of equations will be obtained as follows:

$$\left.\begin{aligned}
&{}^{1}A_x + {}^{1}a_x \cdot {}^{n}A_{x-1} + {}^{n}a_x \cdot {}^{n-1}A_{x-2} + \ldots\ldots {}^{m}a_x \cdot {}^{n-m+1}A_{x-m} = {}^{m+1}a_x ; (1)\\
&{}^{2}A_x + {}^{1}b_x \cdot {}^{1}A_{x-1} + {}^{2}b_x \cdot {}^{n}A_{x-2} + \ldots\ldots {}^{m}b_x \cdot {}^{n-m+1}A_{x-m} = {}^{m+1}b_x ; (2)\\
&\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\\
&{}^{n}A_x + {}^{1}k_x \cdot {}^{n-1}A_{x-1} + {}^{2}k \cdot {}^{n-2}A_{x-2} + \ldots\ldots {}^{m}k_x \cdot {}^{n-m}A_{x-m} = {}^{m+1}k_x ; (n)
\end{aligned}\right\} (C)$$

These equations are of that peculiar class which LAPLACE has distinguished by the name of "re-entering equations" (*equations rentrantes*). The elimination of ${}^{1}A_x, {}^{2}A_x, \ldots {}^{n-m}A_x$ from them may be performed by the ordinary process for elimination between algebraic equations of the first degree, if we only prepare them by substituting $x+1$ for x in (2), $x+2$ for x in (3), and so on throughout; because only one value of each of these functions is contained in each equation, and by this substitution that value becomes the same throughout the system. This done, the system is reduced to another, consisting of m equations, between the remaining m unknown functions, among which the process of elimination must then be pursued by the rules peculiar to equations of differences. Without encumbering our pages with the detail of this operation (which a possible difference in the numerical forms of m and n renders necessarily very complex, when taken up in

its general form) it will be sufficient to state that the final equation for determining ${}^{n}A_x$ will be of the form

$${}^{n}A_{x+mn} + {}^{1}H_x \cdot {}^{n}A_{x+mn-n} + {}^{2}H_x \cdot {}^{n}A_{x+mn-2n} + \ldots {}^{m}H_x \cdot {}^{n}A_x = I_x ; \quad (D)$$

${}^{1}H_x$, ${}^{2}H_x$, &c. and I_x being certain known functions of x.

The integrable cases of this equation (which is an ordinary one of finite differences, and although of the $m \times n^{th}$ order reducible by a very obvious substitution to the m^{th}) are for the most part those in which all the functions ${}^{1}P_x, {}^{2}P_x, \ldots {}^{m}P_x$ are circulating functions with constant coefficients, while ${}^{m+1}P_x$ may be of any form whatever, variable or constant. In these cases then the proposed circulating equation is integrable, and they comprise nearly all which are of any real utility in the present state of analysis.

The complete integral of the equation (D) involves $m.n$ arbitrary constants; and, since by means of the system (C) all the other unknown functions ${}^{1}A_x, {}^{2}A_x, \ldots {}^{n-1}A_x$ may be expressed by linear combinations of ${}^{n}A_x$ and its successive values, these constants will be involved in each of the several terms of which u_x consists. But as their number exceeds what is necessary for expressing the complete integral of the proposed, whose order is only m, there must exist equations of relation between them to the number of $(n-1).m$, or else some of them must coalesce into one, by some peculiarity in the composition of these functions. But any such relations may be directly investigated by substituting the value of u_x with all its superfluous constants in the proposed equation, and causing the result to vanish. It will be worth while,

however, to examine the integrable cases above alluded to rather more minutely.

If we leave out the consideration of the last term ${}^{m+1}P_x$, which is allowable, because by the well known theory of linear equations, the part of the integral dependent on the last term can be supplied afterwards, we have $I_x = 0$, and the rest of the coefficients of the equation (D) constant. Denoting them then by ${}^1H, {}^2H, \ldots\ldots {}^mH$, and supposing α, β, γ, &c. to be the roots of

$$z^m + {}^1H \cdot z^{m-1} + {}^2H \cdot z^{m-2} + \ldots\ldots {}^mH = 0$$

let us take s_x to represent the function

$$\frac{(\sqrt[n]{\alpha})^x + (\sqrt[n]{\beta})^x + (\sqrt[n]{\gamma})^x + \&c.}{m}$$

and the integral of the equation

$${}^nA_{x+mn} + {}^1H \cdot {}^nA_{x+mn-n} + \ldots {}^mH \cdot {}^nA_x = 0$$

may be universally expressed as follows:

$$\begin{aligned}
{}^nA_x = {} & S_x \left\{ {}^1C_1 \cdot s_x + {}^2C_1 \cdot s_{x-n} + {}^3C_1 \cdot s_{x-2n} + \ldots\ldots {}^mC_1 \cdot s_{x-mn+n} \right\} \\
& + S_{x-1} \left\{ {}^1C_2 \cdot s_x + {}^2C_2 \cdot s_{x-n} + \ldots\ldots\ldots\ldots {}^mC_2 \cdot s_{x-mn+n} \right\} \\
& \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\
& + S_{x-n+1} \left\{ {}^1C_n \cdot s_x + {}^2C_n \cdot s_{x-n} + \ldots\ldots\ldots {}^mC_n \cdot s_{x-mn+n} \right\}
\end{aligned}$$

This may be proved without difficulty from the known form of the integral of the equation of a recurring series. Now since the values of ${}^1A_x, {}^2A_x$, &c. are all expressible by means of ${}^nA_x$ as above described, it is easily seen that these are all in like manner circulating functions of a similar form, and since

$$u_x = {}^1A_x \cdot S_x + {}^2A_x \cdot S_{x-1} + \ldots\ldots {}^nA_x \cdot S_{x-n+1}$$

we may neglect in the expression of ${}^1A_x$ all those terms which have not S_x for a multiplier; in that of ${}^2A_x$, all but those multiplied by S_{x-1}; and so on. This is a simplification of considerable moment, and will enable us in any assigned case to dispense with the troublesome and complicated process of determining ${}^2A_x$, &c. by elimination from (C) after ${}^nA_x$ is obtained. In fact, if we suppose (*)

$${}^1A_x = S_x\left\{{}^1C_1 \cdot s_x + {}^2C_1 \cdot s_{x-n} + \cdots {}^mC_1 \cdot s_{x-mn+n}\right\}$$

$${}^2A_x = S_{x-1}\left\{{}^1C_2 \cdot s_x + {}^2C_2 \cdot s_{x-n} + \cdots {}^mC_2 \cdot s_{x-mn+n}\right\}$$

&c.

the relation between the constants which enter into these expressions may be assigned by merely substituting them in the equations (C), and after writing for s_x its value

$$\frac{1}{m}\left\{(\sqrt[n]{\alpha})^x + (\sqrt[n]{\beta})^x + \text{&c.}\right\}$$

and for s_{x-n}, s_{x-2n}, &c. their values.

$$\frac{1}{m}\left\{\frac{(\sqrt[n]{\alpha})^x}{\alpha} + \frac{(\sqrt[n]{\beta})^x}{\beta} + \text{&c.}\right\}, \text{&c.}$$

equating the coefficients of each separate term $(\sqrt[n]{\alpha})^x$, $(\sqrt[n]{\beta})^x$, &c. to zero. This will give $m \times n$ equations of relation between the constants, but it will be found that m of them are necessary consequences of the others, in virtue of the equations (m in number)

* In these equations the constants ${}^1C_1$, ${}^2C_1$, &c. ${}^1C_2$, &c. are not intended to represent the *same* with those in the expression above given for ${}^nA_x$ which are denoted by the same letters. They are functions of them, whose form it is of no moment to enquire, it being sufficient for our purpose, that the one system of constants consists of an equal number with the other, viz. $m \times n$.

$$\alpha^m + {}^1\mathrm{H} \cdot \alpha^{m-1} + \ldots\ldots {}^m\mathrm{H} = 0$$

$$\beta^m + {}^1\mathrm{H} \cdot \beta^{m-1} + \ldots\ldots {}^m\mathrm{H} = 0, \text{ \&c.}$$

The above mentioned equations are therefore equivalent only to $(n-1) \cdot m$ distinct ones, which as we have already seen, is the number of relations which ought to subsist between them. Hence then, all the constants except those which remain arbitrary may be eliminated from the expression of u_x, and the result will be the complete integral required.

(10). Having thus determined the value of u_x, when the last term of the proposed equation, or ${}^{m+1}\mathrm{P}_x$ is zero, if we would then extend the integration to cases where it has any given form, the usual theory of linear equations will afford the requisite formulæ, and their application will be attended with no other embarrassment than what may arise from the integration of explicit functions of the forms

$$\Sigma\, \mathrm{S}_x \cdot f(x) \text{ and } \Sigma\, \mathrm{P}_x \cdot f(x)$$

Into this part of the subject, however, we need not enter at any length, because it may be avoided by pursuing the process originally laid down in (9) without neglecting the last term. We shall confine ourselves to the remark, that both the above expressions are reducible to the form

$$a_x \cdot \mathrm{S}_x + b_x \cdot \mathrm{S}_{x-1} + \ldots . k_x \cdot \mathrm{S}_{x-n+1},$$

and to the developement of one interesting case, viz. that in which $f(x) = 1$, or the function to be integrated is simply S_x.

Let us then denote $\Sigma\, \mathrm{S}_x$ by u_x, and it is remarkable that in the determination of u_x all the ordinary methods are unavailing. It is true that since S_x is of the form

$$\frac{\alpha^x + \beta^x + \gamma^x + \&c.}{n}$$

the direct integration of each term will give

$$u_x = \frac{1}{n}\left\{\frac{\alpha^x}{\alpha - 1} + \frac{\beta^x}{\beta - 1} + \&c.\right\} + C$$

but the peculiar values of α, β, &c. (the roots of unity) render this expression useless. Neither is it of any service to regard S_x as the general term of a recurring series whose equation is

$$S_{x+n} = S_x$$

for the general expression of the sum of such a series becomes illusory when applied to this particular case. We shall be successful, however, if we regard u_x as originating from an equation of differences $\Delta u_x = S_x$, or $u_{x-1} - u_x = S_x$, and treat it as a particular case of the equation integrated in (9). Thus if we assume

$$u_x = {}^1A_x . S_x + {}^2A_x . S_{x-1} + \ldots\ldots {}^nA_x . S_{x-n+1}$$

the system of equations (C) becomes

$${}^2A_{x+1} - {}^1A_x = 1$$
$${}^3A_{x+1} - {}^3A_x = 0$$
$${}^4A_{x+1} - {}^3A_x = 0$$
$$\ldots\ldots\ldots\ldots$$
$${}^1A_{x+1} - {}^nA_x = 0$$

These give

$${}^nA_x = {}^1A_{x+1}, \; {}^{n-1}A_x = {}^nA_{x+1} = {}^1A_{x+2}, \; {}^{n-2}A_x = {}^1A_{x+3}, \ldots\ldots$$
$$\ldots\ldots {}^2A_x = {}^1A_{x+n-1}$$

and finally

$${}^1A_{x+n} - {}^1A_x = 1$$

But, since a particular value of u_x suffices to determine the general one, we need only seek a particular value of ${}^1A_x$:

now such a one presents itself at once, viz. ${}^1A_x = \frac{x}{n}$ because we have obviously $\frac{x+n}{n} - \frac{x}{n} = 1$. Hence then we obtain

$${}^2A_x = \frac{x}{n} + 1 - \frac{1}{n}$$

$${}^3A_x = \frac{x}{n} + 1 - \frac{2}{n}$$

and so on, from which we obtain for the general value of u_x

$$u_x = \text{Const.} + {}^1A_x \cdot S_x + \ldots\ldots {}^nA_x \cdot S_{x-n+1}$$

$$= \text{Const.} + \left\{ S_x + S_{x-1} + \ldots S_{x-n+1} \right\} \left(\frac{x}{n} + 1 \right)$$

$$- \frac{1}{n} \left\{ 0 \cdot S_x + 1 \cdot S_{x-1} + \ldots\ldots (n-1) S_{x-n+1} \right\}$$

But by (6) we have $S_x + S_{x-1} + \ldots\ldots S_{x-n+1} = 1$, and if we include the independent unit in the arbitrary constant, we find

$$u_x = \Sigma S_x = \frac{x - 0 \cdot S_x - 1 \cdot S_{x-1} - 2 \cdot S_{x-2} - \ldots\ldots (n-1) S_{x-n+1}}{n} + C$$

There is something remarkable in this expression. If $x+1$ be put for x, and the integral made to vanish when $x = 0$, it expresses the sum of the series

$$S_1 + S_2 + S_3 + \ldots\ldots S_x$$

this sum will therefore be represented by

$$\frac{x - 0 \cdot S_x - 1 \cdot S_{x-1} - \ldots\ldots (n-1) S_{x-n+1}}{n}.$$

Now it is evident that this series of terms will contain as many equal to unity, as there are units in the integer part of $\frac{x}{n}$, and all the rest are zero. Here then we have an analytical expression for the integer part of the quotient in the division of any one number, x, by any other, n, without in any way specifying their numerical form; whence also a similar expression for the remainder in the same division is easily

obtained: a proposition which seems likely to be of some service in the theory of numbers.

The same expression may also be obtained by the following considerations. If x be a multiple of n, the integer part of $\frac{x}{n}$ will evidently be represented by $\frac{x}{n}$; if x be of the form $in + 1$, or a multiple of n increased by unity, the same integer part will be represented by $\frac{x-1}{n}$; if of the form $in + 2$ by $\frac{x-2}{n}$, and so on. If then we can devise such a function $f(x)$, that when $x = in$ we shall have $f(x) = 0$, when $x = in + 1$, $f(x) = 1$, when $x = in + 2$, $f(x) = 2$, and so on, i being any integer whatever, it is evident that $\frac{x - f(x)}{n}$ will represent in general the integer part of $\frac{x}{n}$. Now

$$0.S_x^{(n)} + 1.S_{x-1}^{(n)} + 2.S_{x-2}^{(n)} + \ldots\ldots (n-1).S_{x-n+1}^{(n)}$$

is such a function. I thought it right to mention this, because the observation of this fact (so deduced) was among the first things which led me to the general consideration of circulating equations in the form I have here presented it.

(11). The next case I propose to consider is that of circulating equations, in which the period of circulation is not the same in all the coefficients. This, however, will not detain us long. Suppose

$$u_x + P_x^{(n)}.u_{x-1} + P_x^{(p)}.u_{x-2} + \ldots\ldots P_x^{(t)}.u_{x-m} = P_x^{(v)}$$

$n, p, q, \ldots\ldots t, v$, denoting the respective periods of circulation in each of the coefficients. Take N to represent the product of all the numbers $n.p.q\ldots\ldots t.v$, divided by their greatest common measure, and $P_x^{(n)}$ may be regarded as a circulating function, whose period of circulation is N, but the

coefficients of which, taken in their order, form $\frac{N}{n}$ subordinate periods within it, each consisting of the n coefficients of $P_x^{(n)}$. Thus suppose $n = 2$, and $N = 6$ then

$$P_x^{(2)} = a_x . S_x^{(2)} + b_x . S_{x-1}^{(2)}$$

$$= a_x . S_x^{(6)} + b_x . S_{x-1}^{(6)} + a_x . S_{x-2}^{(6)} + b_x . S_{x-3}^{(6)} + a_x . S_{x-5}^{(6)} + b_x . S_{x-6}^{(6)}$$

The identity of these two expressions is easily recognised: when x is a multiple of 6, and therefore of 2, they both reduce themselves to a_x; when $x-1$ is such a multiple, to b_x; when $x-2$ is a multiple of 6, $x-2$ is also one of 2, and therefore x is so; consequently both functions reduce themselves to a_x, and so on for every form of x.

Thus every coefficient of the proposed equation may be reduced to one whose period of circulation is N; and this being done, the equation comes under the general form integrated in (9). The second series whose law is stated in Art. (2) leads to an equation of this kind. In fact the equation

$$u_x - (a S_x^{(3)} + b S_{x-1}^{(3)} + c S_{x-2}^{(3)}) u_{x-1} - (\alpha S_x^{(2)} + \beta S_{x-1}^{(2)}) u_{x-2} = 0$$

coincides in succession with the whole series of equations there assigned; and if we write instead of the coefficients of this, the following,

$$a S_x^{(6)} + b S_{x-1}^{(6)} + c S_{x-2}^{(6)} + a S_{x-3}^{(6)} + b S_{x-4}^{(6)} + c S_{x-5}^{(6)}$$

$$\alpha S_x^{(6)} + \beta S_{x-1}^{(6)} + \alpha S_{x-2}^{(6)} + \beta S_{x-3}^{(6)} + \alpha S_{x-4}^{(6)} + \beta S_{x-5}^{(6)}$$

they are reduced to a common period of circulation, and the equation may then be integrated as above. It remains to consider equations with more than one independent variable whose coefficients are circulating functions.

(12). A circulating function with two independent variables may have a double period of circulation, and may in general be represented thus:

$$P^{(m,n)}_{x,y} = a^{(0,0)}_{x,y} S^{(m)}_{x} S^{(n)}_{y} + a^{(1,0)}_{x,y} S^{(m)}_{x-1} S^{(n)}_{y} + a^{(0,1)}_{x,y} S^{(m)}_{x} S^{(n)}_{y-1} + \ldots\ldots\ldots + a^{(m-1,n-1)} S^{(m)}_{x-m+1} S^{(n)}_{y-n+1}$$

one with three may have a triple one, and its expression will be

$$P^{(m,n,r)}_{x,y,z} = a^{(0,0,0)}_{x,y,z} S^{(m)}_{x} S^{(n)}_{y} S^{(r)}_{z} + a^{(1,0,0)}_{x,y,z} S^{(m)}_{x-1} S^{(n)}_{y} S^{(r)}_{z} + \&c.$$

and so on, and these functions possess general properties analogous to those of one variable. To avoid complication we will confine ourselves to the case of two variables, a similar mode of treatment being applicable to any number.

First then, any function whatever of x, y, $S^{(m)}_{x}$, $S^{(n)}_{y}$, &c. and their successive values is reducible to the form $P^{(m,n)}_{x,y}$ and thus the sums, products, quotients, or powers of two or more such functions are reducible to a single one, as in the case of one variable.

2dly. Any symmetrical function whatever of the values

$$P^{(m,n)}_{x,y},\ P^{(m,n)}_{x-1,y},\ P^{(m,n)}_{x,y-1}, \ldots\ldots\ldots\ldots\ldots\ P^{(m,n)}_{x-m+1,y-n+1}$$

is invariable, provided the coefficients of each term in the expression of $P^{(m,n)}_{x,y}$ are so; and it is equal to a function similarly composed of those coefficients.

3dly. Any circulating function of the form $P^{(m,n)}_{x,y}$ may be reduced to another of the form $P^{(M,N)}_{x,y}$, M and N being any multiples of m and n respectively, and thus any number of such functions may be reduced to the same period of circula-

tion. These properties are easily demonstrated as in the case of a single independent variable, and by their aid all circulating equations of partial differences may be cleared of their circulating form.

(18). Let the proposed equation of partial differences be

$$u_{x,y} + {}^{(1,0)}\mathrm{P}^{(m,n)}_{x,y} \cdot u_{x-1,y} + {}^{(0,1)}\mathrm{P}^{(m,n)}_{x,y} \cdot u_{x,y-1} + \ldots\ldots$$
$$\ldots\ldots {}^{(r-1,s-1)}\mathrm{P}^{(m,n)}_{x,y} \cdot u_{x-r,y-s} = \mathrm{P}^{(m,n)}_{x,y}$$

To integrate it, we assume

$$u_{x,y} = \mathrm{A}^{(0,0)}_{x,y} \cdot \mathrm{S}^{(m)}_{x}\,\mathrm{S}^{(n)}_{y} + \mathrm{A}^{(1,0)}_{x,y} \cdot \mathrm{S}^{(m)}_{x-1}\,\mathrm{S}^{(n)}_{y} + \mathrm{A}^{(0,1)}_{x,y} \cdot \mathrm{S}^{(m)}_{x}\,\mathrm{S}^{(n)}_{y-1} +$$
$$\ldots\ldots\ldots\ldots + \mathrm{A}^{(m-1,n-1)}_{x,y} \cdot \mathrm{S}^{(m)}_{x-m+1}\,\mathrm{S}^{(n)}_{y-n+1}$$

and supposing the general representation of any one of the coefficients as ${}^{(r,s)}\mathrm{P}^{(m,n)}_{x,y}$ to be

$${}^{(r,s)}a^{(0,0)}_{x,y} \cdot \mathrm{S}^{(m)}_{x}\,\mathrm{S}^{(n)}_{y} + {}^{(r,s)}a^{(1,0)}_{x,y} \cdot \mathrm{S}^{(m)}_{x-1}\,\mathrm{S}^{(n)}_{y} + {}^{(r,s)}a^{(0,1)}_{x,y} \cdot \mathrm{S}^{(m)}_{x}\,\mathrm{S}^{(n)}_{y-1} + \&c.$$

Then, if the above expression be substituted for u_x in the proposed equation, it becomes

$$= \mathrm{S}^{(m)}_{x}\,\mathrm{S}^{(n)}_{y} \cdot \left\{ \mathrm{A}^{(0,0)}_{x,y} + {}^{(1,0)}a^{(0,0)}_{x,y} \cdot \mathrm{A}^{(m-1,0)}_{x-1,y} + {}^{(0,1)}a^{(0,0)}_{x,y} \cdot \mathrm{A}^{(0,n-1)}_{x,y-1} + \&c. - a^{(0,0)}_{x,y} \right\}$$
$$+ \mathrm{S}^{(m)}_{x-1}\,\mathrm{S}^{(n)}_{y} \cdot \left\{ \mathrm{A}^{(1,0)}_{x,y} + {}^{(1,0)}a^{(1,0)}_{x,y} \cdot \mathrm{A}^{(0,0)}_{x-1,y} + {}^{(0,1)}a^{(1,0)}_{x,y} \cdot \mathrm{A}^{(1,n-1)}_{x,y-1} + \&c. - a^{(1,0)}_{x,y} \right\}$$
$$+ \mathrm{S}^{(m)}_{x}\,\mathrm{S}^{(n)}_{y-1} \cdot \left\{ \mathrm{A}^{(0,1)}_{x,y} + {}^{(1,0)}a^{(0,1)}_{x,y} \cdot \mathrm{A}^{(m-1,1)}_{x-1,y} + {}^{(0,1)}a^{(0,1)}_{x,y} \cdot \mathrm{A}^{(0,0)}_{x,y-1} + \&c. - a^{(0,1)}_{x,y} \right\}$$
$$\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$
$$+ \mathrm{S}^{(m)}_{x-m+1}\,\mathrm{S}^{(n)}_{y-n+1} \cdot \left\{ \mathrm{A}^{(m-1,n-1)}_{x,y} + {}^{(1,0)}a^{(m-1,n-1)}_{x,y} \cdot \mathrm{A}^{(m-2,n-1)}_{x-1,y} + \&c. - a^{(m-1,n-1)}_{x,y} \right\}$$

Let now each term of this, enclosed in the brackets, be separately made to vanish, and we shall obtain a system of *re-entering equations* of partial differences, for the determination of the unknown functions $\mathrm{A}^{(0,0)}_{x,y}$, &c. The number of these equations is $m \times n$, which being also that of the functions to be determined, they suffice for the purpose.

It will be unnecessary to enter into the detail of the process of elimination in this case; it is always practicable, and leads to a final equation with constant coefficients, when those of the circulating functions which enter into the proposed equation are constant, just as in the case of one variable.

(14). I am unwilling to occupy the pages of the Philosophical Transactions with examples of the application of the processes here delivered to the various problems in the pure and mixed mathematics where they afford either a remarkable simplicity in the result, or great neatness in the investigation. Such instances occur frequently in the evaluation of continued fractions and other similar functions where the denominators (or other elements) recur in a certain order. A variety of complicated questions relative to the simultaneous employment of capital in different mercantile transactions, can scarcely be treated with perspicuity in any other way, and other instances will readily suggest themselves to the reader whose experience in enquiries of this nature has led him to feel the inconvenience which these pages are designed to obviate. I will therefore merely subjoin one example of the integration of a circulating equation of the second order, with constant coefficients, by way of illustration of the methods themselves.

Suppose then we have

$$u_x - (a\mathrm{S}_x + b\mathrm{S}_{x-1})u_{x-1} + (\alpha\mathrm{S}_x + \beta\mathrm{S}_{x-1})u_{x-2} = 0$$

where S_x is $\frac{1}{2}$ the sum of the x^{th} powers of the roots of $z^2 - 1 = 0$ assume

$$u_x = \mathrm{A}_x \cdot \mathrm{S}_x + \mathrm{B}_x \cdot \mathrm{S}_{x-1}$$

and we get by substitution

$$0 = S_x\{A_x - aB_{x-1} + \alpha A_{x-2}\} + S_{x-1}\{B_x - bA_{x-1} + \beta B_{x-2}\}$$

whence we find

$$A_x + \alpha A_{x-2} = aB_{x-1}$$
$$B_x + \beta B_{x-2} = bA_{x-1}$$

The first of these gives $B_x = \frac{1}{a}\{A_{x+1} + \alpha A_{x-1}\}$; (E)

which substituted in the second multiplied by a produces

$$A_{x+1} + (\alpha + \beta - ab) A_{x-1} + \alpha\beta A_{x-3} = 0$$

or, $$A_{x+1} - (ab - \alpha - \beta) A_{x+2} + \alpha\beta . A_x = 0$$

This equation corresponds to the equation (D) of the foregoing articles, and if we take p and q such that

$$(z^2 - p^2)(z^2 - q^2) = z^4 - (ab - \alpha - \beta) z^2 + \alpha\beta,$$

we shall have for the complete value of A_x

$$\left.\begin{aligned} A_x = S_x\{ C . (p^x + q^x) + C' . (p^{x-2} + q^{x-2}) \} \\ + S_{x-1}\{ c . (p^x + q^x) + c' . (p^{x-2} + q^{x-2}) \} \end{aligned}\right\} \quad (F)$$

This expression substituted in (E) will give the value of B_x, but as it is sufficiently evident that the expression so obtained, as well as the value of u_x thence derived, will all have the same form, the constants only differing, we may at once suppose u_x equal to the second member of (F) and determine the relations between C, C', c, c' by substituting it in the proposed equation. This gives the following four equations of condition among the constants.

$$\left.\begin{aligned} (C + \tfrac{C'}{p^2})(1 + \tfrac{a}{p^2}) - \tfrac{a}{p}(c + \tfrac{c'}{p^2}) = 0 \\ (c + \tfrac{c'}{p^2})(1 + \tfrac{\beta}{p^2}) - \tfrac{b}{p}(C + \tfrac{C'}{p^2}) = 0 \end{aligned}\right\}$$

$$\left.\begin{aligned} (C + \tfrac{C'}{q^2})(1 + \tfrac{a}{q^2}) - \tfrac{a}{q}(c + \tfrac{c'}{q^2}) = 0 \\ (c + \tfrac{c'}{q^2})(1 + \tfrac{\beta}{q^2}) - \tfrac{b}{q}(C + \tfrac{C'}{q^2}) = 0 \end{aligned}\right\}$$

Eliminating $(c + \frac{c'}{p^2})$ from the two first of these, and $(c + \frac{c'}{q^2})$ from the two last we find

$$\frac{1}{p^4} \cdot \left(C + \frac{C'}{p^2}\right) \cdot \left\{p^4 - (ab - \alpha - \beta)\, p^2 + \alpha\beta\right\} = 0$$

$$\frac{1}{q^4} \cdot \left(C + \frac{C'}{q^2}\right) \cdot \left\{q^4 - (ab - \alpha - \beta)\, q^2 + \alpha\beta\right\} = 0$$

but the values of p and q are such by hypothesis, that the latter factors of these equations vanish separately of themselves: the equations are therefore verified independently of any particular values of $C + \frac{C'}{p^2}$ or $C + \frac{C'}{q^2}$, which therefore remain arbitrary. C and C′ then being arbitrarily assumed, and c, and c' determined from the above equations, which give

$$c = \frac{C\,pq\,(p^2 + q^2 + pq + \alpha) + C'\,(pq - \alpha)}{\alpha \cdot pq \cdot (p + q)}$$

$$c' = \frac{C'\left\{(pq)^2 + \alpha\,(p^2 + q^2 + pq)\right\} - C\,(pq)^2 \cdot (pq - \alpha)}{\alpha \cdot pq \cdot (p + q)}$$

the second member of the equation (F) will be the complete value of u_x, and it will be noticed that we have

$$pq = \sqrt{\alpha\beta}\,,\; p^2 + q^2 = ab - (\alpha + \beta)$$
$$(p + q) = \sqrt{(p^2 + q^2) + 2pq}$$

whence the whole is readily reduced to functions of the proposed coefficients a, b, α, β.

JOHN F. W. HERSCHEL.

Slough, Nov. 1817.

X. *On the fallacy of the experiments in which water is said to have been formed by the decomposition of Chlorine.* By Sir H. Davy, *LL. D. F. R. S.*

Read February 12, 1818.

Some experiments have been lately communicated to the Royal Society of Edinburgh, from which it has been inferred, that water is formed during the action of muriatic acid gas on certain metals, and consequently, that chlorine is decomposed in this operation.

In repeating those experiments, I have ascertained, that the water is derived from sources not suspected by the authors, and that their conclusions are unfounded. To take up the time of the Society by long experimental details and theoretical speculations on such an occasion, will be unnecessary; I shall therefore only transiently mention the sources of error, and demonstrate their operation by two or three examples.

When muriatic acid gas is passed through flint glass tubes heated to redness, a small quantity of water is formed by the action of the gas on the oxide of lead in the glass, and a smaller quantity by its action on the alkali of the glass, the process being one of double affinity, the hydrogen of the muriatic acid unites to the oxygen of the oxide, and the chlorine combines with the metals.

A copious dew was formed by passing muriatic acid gas through flint glass tubes red hot, and a less copious dew, by passing it through green glass tubes. In the first instance,

the glass became opaque, and gained a pearly lustre, and a combination of chlorine and lead sublimed from the hotter into the colder part of the tube. In the second, the surface of the tube became slightly opaque, but no sublimate was formed.

When fine clean iron wire was introduced into such tubes, and made red hot, and muriatic acid gas passed over it, no particular precautions being taken to free the tubes from common air, much more water appeared; but this excess of water principally owed its existence to the combination of hydrogen disengaged from the muriatic acid gas by the iron with the oxygen of the common air. I say, *principally*, because an inappreciable quantity must have been deposited from the vapour of hydrated muriatic acid in the muriatic acid gas. This was proved by filling the whole apparatus with hydrogen in another experiment, and generating the muriatic acid gas in a retort filled with hydrogen, when the water produced was no more than might have been expected from the action of the muriatic acid gas on the oxide of lead and alkali in the glass. I give the details. Above 21 grains of the first combination of chlorine and iron were formed; the quantity of moisture collected by bibulous paper, and which was a strong acid solution of the proto-muriate of iron, amounted to less than half a grain, and of this not more than two-thirds could have been water. Now, if chlorine had been decomposed in this operation, the quantity of water ought to have been at least ten times as great.

I have shown by numerous experiments, that in the action of muriatic acid gas upon metals, hydrogen, equal in bulk to half the volume of the gas, is produced; it is therefore evident, that if water had been generated by the action of mu-

riatic acid gas on metals, it must have been the *chlorine*, or the *metal*, or both, that were decomposed. As chlorine can be freed from much of its aqueous vapour by dry muriate of lime, which is not the case with muriatic acid gas, it offers a much more unexceptionable substance for experiments of this kind. I passed 23 cubical inches of chlorine slowly through dry muriate of lime into a flint glass tube red hot, containing a green glass tube full of iron wire; the chlorine combined with this iron wire with intense heat; the bright sublimate formed was passed through more iron wire heated to redness, so as to form a considerable quantity of the first compound of chlorine with iron, which, when examined, was found exactly the same as that produced by the action of muriatic acid gas on iron. All the products were heated strongly, and the end of the glass tube kept very cool; but *not the slightest appearance of moisture was perceptible.*

In all these experiments I was assisted by Mr. FARADAY of the Royal Institution.

Muriate of ammonia is not altered by being passed through porcelain or glass tubes heated to redness, but if metals be present, it offers similar results to muriatic acid gas. In one experiment, in which muriate of ammonia recently sublimed was used, instead of muriatic acid gas, the appearance of moisture was less than in the experiment on muriatic acid gas, which has been just detailed, and yet there was a considerable action on the oxide of lead in the glass, not only by the muriatic acid, but likewise by the free hydrogen of the decomposed ammonia.

XI. *The Croonian Lecture. On the changes the blood undergoes in the act of coagulation. By Sir* Everard Home, *Bart. V. P. R. S.*

Read November 20, 1817.

It is not a little remarkable, that in the first Lecture of this kind, which I laid before the Society, in the year 1790, I should have endeavoured to show, that a muscular fibre was too minute an object to be seen by the human eye, even assisted by the best magnifying glasses then in use; and that in this Lecture, I shall be able, by means of the great improvements that have been made in the use of the microscope, to show that a fibre not larger in diameter than one of the globules of the blood can be demonstrated.

To the Members of this Society who have so lately seen Mr. Bauer's drawings, of the glandular apparatus peculiar to the Java swallow; of the internal membrane of the human stomach, exposing structures that were not known to exist; also of so small an object as the human ovum, in which is seen the seat of the two most important organs of the body; (drawings rendered beautiful by their simplicity and distinctness;) it will readily suggest itself, that Mr. Bauer is the person to whom I consider we are indebted for those improvements. His whole life, I may say, has been employed in investigations of a similar nature in plants, observing first the natural appearances, and then magnifying them in different

degrees, and comparing, with the nicest discrimination, what was exhibited by one magnifying power, with what was shown by that immediately above it, and, where they did not exactly correspond, employing the whole energies of his mind, with a patient labour, almost beyond what is natural, in ascertaining the cause of the deception which must in one of them have taken place. To the observations of such a man upon subjects of this nature, if we are not confidently to place a reliance, how are we to give credit to the remarks that are made by common observers?

I have said thus much as an introduction to the observations that I am going to bring forward, for the public to know, whatever opinion they may form of them, they have been the result of long and unwearied research; and have been so frequently repeated as to satisfy Mr. BAUER of their correctness.

The red globules of the blood in the human body, when enveloped in their colouring matter, appear, when measured in the microscope by the micrometer, to be $\frac{1}{1700}$ part of an inch in diameter, requiring 2,890,000 to a superficial or square inch. These globules, when deprived of their colouring matter, appear to be $\frac{1}{2000}$ part of an inch in diameter, which makes 4,000,000 of globules to a square inch. From these observations, it appears that the globules, when deprived of the colouring matter, are not quite one fifth part smaller. The colouring substance appears not to be contained in the globules, but only to envelope them: one reason for forming this opinion is, that the separation is very rapidly effected, the colouring substance flowing from all parts of the globule at the same instant, and that to retain the globules in the coloured state it is necessary that a very small quantity of blood only be

smeared as thin as possible upon the glass, in order that all moisture may instantly evaporate; they then remain of their full size and colour, perfectly spherical, as in the representation, fig. 1, Pl. viii. But if a greater quantity of blood be laid upon a glass which retains moisture only half a minute, the colouring matter begins, in a few seconds, to separate and form a circle round the globule, and if the blood is diluted with water, the separation of the colouring matter is instantaneous, and the globule puts on the appearance represented in fig. 2, Pl. viii. Another reason is, the great quantity of colouring matter, it being as three to one in proportion to the globules.

The globules of the blood have neither the same size nor the same shape in all animals. Dr. YOUNG, in his introduction to Medical Literature, has described them to be of an oval form in the skate. Upon examining them in that fish, Mr. BAUER found them, while the fish was alive, of the form of an egg, but almost immediately after death, flattened. I shall, however, reserve the materials I have collected upon this subject for another communication. When the globules in the human blood lose the colouring matter, they continue floating in the serum, and are seen to have an attraction towards one another so as to coalesce, uniting themselves together. In the annexed drawing is represented their mode of union under different circumstances, surrounded by serum deeply tinged with the colouring matter. In one instance three globules are so united as to form one line; in another there is a line composed of four globules, with lateral indentations, where the union between the globules had taken place. This appearance, joined to other circumstances, renders it probable at least that the globules may be the part of the blood, out of which

the muscular fibres are principally formed, and no fibre they could form would be of smaller dimensions than the globules of which it is composed. Having expressed this opinion to Mr. Bauer, I was desirous that he should try to unravel a muscle so as to come at the ultimate fibre. Several attempts of this kind were unsuccessfully made: but in one instance, in the muscles of the thigh of a roasted chicken, a detached fibre was exposed in the microscope, which occupied upon the micrometer, the same space in every respect with the four globules which I have mentioned to have seen united, and floating in the serum of the blood before the blood had completely coagulated; the muscular fibre from the chicken could be traced to a greater length, but the indentations could not be distinguished farther on.

In prosecuting this examination of the muscular fibres, Mr. Bauer found that after being boiled or roasted, and then macerated in water, changing the water every day for a week, they were much more readily separated from each other, and that he had no difficulty in procuring single fibres similar to the one described, from the coats of the human stomach; the thigh of the sheep, and of the rabbit; and from the salmon. The appearances that different fibres put on are represented in the annexed figures, 4, 5, 6, of Plate viii.

When the fibres are macerated for a longer time, they are readily broken down into a mass of globules of the size of those in the blood, deprived of their colour. The accuracy of the appearances that have been described may be depended on; how far they will afford the slightest grounds for an opinion that the globules are the materials, and the attraction between them the means, by which the single fibres are formed, and all the

combinations produced that are met with in the structure of muscles, must require farther investigation. It is deserving of remark, that while the globules are enveloped in their colouring matter, they are not seen to run together, and coalesce with one another on the field of the microscope; it is therefore probable that the attraction, by which this effect is produced, only takes place between globules deprived of their colour.

It may not be amiss to enquire how far there is any thing in the formation of fibres in other parts of the body at all in favour of muscular fibres being composed of globules, and I shall mention that Mr. BAUER, in his examination of the substance of the brain, under the microscope, finds, that when that organ is immediately after death made the subject of examination, abundance of fibres are met with in every part of it; indeed it appears that the whole mass is a tissue of fibres, which seems to consist entirely of an accumulation of globules, whose union is of so exceedingly delicate a nature, that the slightest touch, even the mere suction in water, deranges and reduces them to that mass of globules of which the brain appears to be composed when examined with less accuracy, or under less favourable circumstances. He admits that in his first observations he was induced to believe that no such things as fibres were to be met with in the brain, but that the whole organ consisted of a mass of these globules. He found that the globules of the brain, as well as those of pus, are exactly the same size as those of the blood when deprived of their colouring matter.

Mr. BAUER not having completed the investigation of the brain, in which he is engaged, I shall not farther anticipate

his observations upon this most important organ: as my only object in what I have already stated, is to mention, that the fibres of which it appears to be made up are composed of globules.

Upon mentioning Mr. BAUER's observations on the brain to some of my friends, I was referred to the Supplements to the 4th and 5th editions of the Encyclopædia Britannica,* in which the opinion of the brain being composed of globules is noticed. I find, upon making such reference, that there is not enough stated respecting the anatomy of the more minute parts of that organ, to supersede a farther investigation of its structure, as will appear from the following extracts.

"It (the brain) has been subjected to very minute microscopical observations by PROCHASKA.† When he took a small portion of it, either from the brain proper or the cerebellum, and spread it on a thin plate of glass, so that it became pellucid, and then examined it with a powerful microscope, he found that it resembled a sort of pulp, consisting of innumerable globules, or particles of a roundish form. A little water added to this pulp, divided it into a number of flocculi; but he observed that each flocculus was still composed of a number of globules. He very rarely found one globule by itself, or even two, floating in water, apart from the rest. Maceration in water, even for three months, was insufficient to separate them from each other. He concluded, therefore, that they were united by means of a very delicate and pellucid cellular substance. The globules, he observed, were not all of the

* Vol. 1, part 2, page 260, No. 64 and No. 65.

† Oper. Min. Pars. I. p. 342.

same size; but varied a little in dimension, even in the same part of the brain. In general, however, he found them, both in the brain proper and in the cerebellum, to be more than eight times smaller than a globule of the blood. The most powerful microscopes did not enable him to discover any thing satisfactory respecting their structure."

"These observations have, within these few years, been prosecuted on a much more extensive scale, by JOSEPH and CHARLES WENZEL.* They have uniformly found, that the white nervous matter seemed as if entirely composed of extremely small globules or corpuscules of a roundish form, putting on the appearance of little cells, filled with a proper medullary substance. No estimate is given of the dimensions of the globules, but they describe them as being exceedingly minute, and as being all pretty nearly of the same size. They seemed to adhere very closely to each other, without any apparent connecting medium. The globular appearance continued distinctly perceptible in portions of the substance, which had been long exposed to the action of rectified spirit of wine and muriatic acid; nor was it even destroyed by steeping the matter in alcohol, and then drying it."

The statements contained in both of these extracts confirm, in the most satisfactory manner, MR. BAUER's observations, although in many respects, they are deficient in point of accuracy.

Having laid before the Society all I have to offer, respecting the appearances of the globules of the blood from which the colouring matter had been discharged, I shall endeavour to explain in what manner blood, in the act of coagulation,

* De Penitior. Struct. Cereb. p. 24.

acquires that texture, which fits it, when extravasated in living animals, to open a communication with the general circulation, and by that means become a part of the solids of the animal.

It has ever been a desideratum to ascertain in what manner blood after it had coagulated, and remained at rest in different parts of a living animal, is rendered vascular. The fact itself has long been known to every enquirer into the operations of the animal economy, and several theories have been formed to explain it. Mr. HUNTER, who perhaps understood the appearances such coagula put on, when injected from the neighbouring vessels, better than any other physiologist, was unable to trace a direct continuation of ramifying branches from the circumference of the living parts to the centre of the coagulum, and therefore referred it to a principle of life existing in the blood, which principle was consequently inherent in the coagulum, and formed a series of vessels, pervading every part of it, which opened for themselves communications with the surrounding vessels. Since Mr. HUNTER's time, no more satisfactory opinion has been advanced for the explanation of this curious phenomenon. I confess that my own attention has not, for the last twenty years, been called to this enquiry, although before that time, while I was assisting Mr. HUNTER in the prosecution of his pursuits, I gave considerable attention to it, but remained unsatisfied with all the explanations that had been given.

My attention was, however, again called to this subject last summer, by different conversations I had with Mr. BAUER, in which he told me, that to illustrate the germination and vegeta-

tion of wheat, introductory to his illustrations of the diseases in corn, he sowed a quantity of wheat, and afterwards took up every day some grains, or plants, for examination, till the ears were ripe. In his close attention to the changes that took place, he was very much struck with the rapid increase of the tubular hair of the root of a young plant of wheat, in its earliest stage of vegetation: and fixing his whole attention upon that part of the plant, he observed small pustules of a slimy substance arising under the epidermis, on the surface of the young root; and, in a few seconds, a small bubble of gas burst from the root into the slimy matter, which it extended in a moment to the length the hair was to acquire; and the slimy matter, surrounding the gas, immediately coagulated, and formed a canal. He repeated his observations on another plant, whose pubescence consisted of a jointed hair, and observed the same effect take place; a bubble issued from the young stalk, and extended the slimy mucus to a short distance, forming the first joint, which immediately coagulated and became transparent, and at its extremity a new pustule of the same slimy mucus accumulated, into which, in a short time, the gas from the first joint rushed; and thus, in a moment, a second joint was formed: in the same manner he observed the formation of the hairs of ten or twelve joints to take place.

These observations, so curious in themselves, and which explain, in so simple and satisfactory a manner, one of the modes in which tubes are formed in vegetables, and an addition is made to the plant, made so strong an impression on my mind, and so entirely engrossed my attention, that I did

not allow Mr. BAUER to rest, till he gave me his assistance in instituting experiments, to ascertain whether any thing similar takes place in animal bodies.

The first object of our enquiry was to know, whether any gas is to be found in the blood while circulating in the vessels, and under what circumstances it is separated from it. That the blood, whilst circulating in the arteries and veins, holds a considerable quantity of gas in solution, is proved by the following experiments, made at my request, by Professor BRANDE. Blood was drawn from a vein in the arm, and whilst yet warm was placed under the receiver of an air pump; during the exhaustion of the receiver, there was a considerable escape of gas from the blood, so that it had the appearance of effervescing, and soon depressed the quicksilver in the gage of the pump. He afterwards ascertained that this gas is carbonic acid gas, is met with in the same proportions in arterial and venal blood, and two cubic inches were extricated from every ounce of blood.*

That a considerable portion of this carbonic acid gas, is extricated from the blood during the spontaneous coagulation of that fluid, was previously proved by Mr. BAUER, who filled glass tubes with blood recently drawn, and tying them over with bladder, inverted them. At first there was no appearance of gas upon the surface, but as the blood coagulated, it was separated, and in the course of 24 hours was found in considerable quantity.

Having ascertained not only the existence of gas in the blood, but that it is separated during the process of coagulation, I was most anxious to discover whether, as in vegetables, the gas,

* See Annales de Chimie, Tom. XIII.

thus let loose, pervaded the surrounding fluid into which it is propelled, in any particular manner. With a view to determine this point, I wounded the skin of my arm with the point of a lancet, so as to draw a drop of blood, which was received into a watch glass in a fluid state, and placed in the field of the microscope. The eye was then kept constantly fixed upon it, to watch the changes that might take place. The first thing that happened, was the formation of a film upon the surface, that part beginning to coagulate sooner than the rest. In about five minutes, something was seen to be disengaged in different parts of the coagulum, beginning to show itself where the greatest number of globules were collected; and from thence passing in every direction with considerable rapidity through the serum, but not at all interfering with the globules themselves, which had all discharged their colouring matter; wherever this extricated matter was carried, a net-work immediately formed, anastomosing with itself on every side, through every part of the coagulum. When the parts became dry, the appearance of a net-work remained unaltered. In some instances, bubbles were seen to burst through the upper surface of the coagulum: this, however, did not prevent the ramifications that have been described from taking place. The annexed Plates give the exact representation of the appearance the blood taken from my arm put on, when it coagulated and became dry, as shown in the field of the microscope. [Pl. ix, x.]

If the blood is cold when it is exposed in the microscope, and there is a larger quantity of serum upon the glass, the net-work is only formed in those parts where clusters of globules are collected; and when the serum dries, it cracks, and spoils

the appearance. This happens sometimes several days after the formation of the net-work has taken place. When clear serum without any globules is put upon the glass, nothing is extricated, but when the serum is quite dry, it cracks, and the cracks may be mistaken for the net-work; but by comparing them with it, the difference is found to be obvious.

These facts which Mr. BAUER has enabled me to bring forward, appear to point out an important change the blood undergoes, after it is extravasated. When this happens in living animal bodies, from whatever cause, and in whatever circumstances it takes place, no difficulty remains in accounting for its afterwards becoming vascular, since all that is necessary for that purpose is the red blood being received into the channels of which this net-work is formed.

I shall not detain the Society with any farther observations in support of what I have advanced, satisfied that if the facts do not bear themselves out, it will be superfluous to load them with theoretical opinions.

I cannot conclude this Lecture, without paying a tribute to the President of the Society; not a tribute of praise, but a tribute of justice; for whenever general science, or any of its branches, are brought under his consideration, the zeal and exertion, which he shows upon all occasions, to promote the pursuits of individuals, exceeds whatever has been done by others, in this or any other country, and is above all praise.

Whatever Mr. BAUER has already done, and whatever he may hereafter bring to light, respecting the more minute parts of animal bodies, is entirely to be attributed to the President; for it is at his particular request, under his encouragement, and in compliance with his wishes, that Mr.

BAUER has put a restraint upon himself, and for a time laid aside the prosecution of the Anatomy of Vegetables, which from his early youth has been his favourite occupation, to assist in bringing to light appearances in the anatomy of animals, which, without his aid, must still have remained in obscurity.

XII. *Some additions to the Croonian Lecture, on the changes the blood undergoes in the act of coagulation.* By Sir Everard Home, *Bart. V. P. R. S.*

Read March 5, 1818.

Several of my friends, much more deeply versed in mathematics than myself, who were present at the reading of the Croonian Lecture, remarked that no spherical bodies could be accurately measured by the common micrometer, and therefore no correct idea of the diameter of a globule of the blood could be obtained by that means. They were also led to doubt the appearance represented in the coagulum, being real, since air, in all ordinary circumstances, when let loose, forms itself into globules, not moving in straight or curved lines.

These objections, coming from philosophers for whose opinions on such subjects I have the highest respect, induced me to request permission of the President to withdraw the Lecture, that I might correct any errors I had fallen into before the Paper came before the Committee. I found also upon reflection, that I had left the investigation more imperfect than I was aware of, since it is of very little consequence whether, in the act of drying, coagulated blood puts on this particular appearance or not, if I cannot at the same time adduce proofs of the same changes taking place in coagula while they are still moist, and also in the blood when it is

extravasated, and coagulates in the interior parts of living animals.

I have therefore reconsidered this subject, and with the assistance of Mr. BAUER, have instituted a series of experiments, and had drawings taken to elucidate their results, which I hope will do away the objections made against coagulated blood having channels formed through it by the extrication of carbonic acid gas; and prove, not only that the same change takes place when blood is extravasated in living animal bodies, but that these channels have a communication opened between them and the neighbouring arteries, and that the fluid blood circulates through the channels in the coagulum. In laying these experiments and drawings before the Society, I request that I may be indulged in adding them to the Croonian Lecture; and with these additions, that it may be submitted to the judgment of the Council.

As the measurement of spherical bodies is a subject on which I am totally unfit to form an opinion, I requested my friend Captain KATER to have the goodness to measure the diameter of a globule of the blood, in what appeared to him the most satisfactory manner, and to explain to me the mode of doing it. He very readily complied with my request, and the following is the mode which he adopted.

A ruler divided into inches and tenths was placed on the box which supports the microscope, a mother of pearl micrometer scale was placed under the microscope, each division of which was equal to one two-hundredth of an inch: viewing this with both eyes open, its image appeared to be projected on the ruler, and one division appeared to subtend the space of one inch. The micrometer scale being removed, blood

sufficiently dilute was placed under the microscope, and being viewed with both eyes open, a globule of blood appeared to occupy, in the first experiment, one half of one tenth of an inch, and in the second experiment, one third of one tenth of an inch upon the ruler. Hence the size of the globule by the first experiment will be equal to

$\frac{1}{2}$ of $\frac{1}{10}$ of $\frac{1}{200}$ of 1 inch $= \frac{1}{4000}$ of an inch;

and by the second experiment

$\frac{1}{3}$ of $\frac{1}{10}$ of $\frac{1}{300}$ of 1 inch $= \frac{1}{9000}$ of an inch;

the mean of which, or $\frac{1}{6000}$ of an inch, may be considered as about the mean diameter of a globule of the blood.

This measurement of Captain KATER's, as it was natural to expect, corresponds with that which has been made by Dr. WOLLASTON, by means of a very ingenious micrometer of his own invention, a description of which has a place in the Philosophical Transactions; and with the measurement of Dr. YOUNG in his eirometer, of which he has given an account in his Introduction to Medical Education.

The diameter of a globule of the blood, measured by mathematicians of such eminence, is to be set down as $\frac{1}{1000}$ part of an inch; and the diameter in the micrometer, measured with all the accuracy that instrument is capable of, since such was the smallest apparent dimension which occurred in Mr. BAUER's experiments, as $\frac{1}{1000}$ part of an inch.

I have taken more pains to have the difference between the measurement of a globule in these different modes ascertained, than the subject would appear to require; but its being known, will enable microscopical observers, unskilled in the higher branches of mathematics, to pursue their observations upon globules of different sizes, and continue to compare their re-

lative size in the micrometer, which will give a correct result which ever mode of measurement is adopted respecting the original globule.

To do away the objection which has been made to gas being contained in the net-work formed in coagulated blood, I first made the following experiment. I placed a vessel nearly filled with blood drawn from the arm, under the receiver of an air pump, and by exhaustion extracted the gas contained in the blood. This blood deprived of its gas when coagulated, exhibited no appearance of net-work. In that part which had coagulated before the exhaustion was completed, the net-work was beautifully distinct.

When blood is drawn from the arm into a cup, and allowed to stand 48 hours, the serum is separated, and every where encloses the coagulum. The greater part of the surface of the coagulum is covered with small round holes in which the gas had collected, and then forced its way out into the serum. But if blood taken by cupping is allowed to stand 48 hours in a cup, sometimes the serum is only separated in small quantity, and does not rise above the coagulum, in consequence of a film or pellicle forming on the surface of the coagulum, and fixing itself to the edge of the cup all round. This pellicle when examined at the end of 48 hours appears to contain ramifying vessels. This arises from the mode by which the blood is extracted depriving it of a part of its carbonic acid gas, and what remains is not sufficient in quantity to burst the pellicle, and when in the act of extrication it arrives immediately under the pellicle, it is forced to spread in different directions, putting on this appearance.

Having ascertained that this appearance is produced by the

extrication of carbonic acid gas, I was led to make the attempt to inject the coagulum with the common minute injection under the receiver of an air pump. The experiment was made in the following manner. A glass cup about an inch and a half deep, and nearly three inches in diameter, had blood from the arm drawn into it, till it was three parts full. The blood was allowed to stand in a cool place for 48 hours, the serum was then strained off, and about $\frac{1}{4}$ of the coagulum on one side was cut away and removed, and the cavity thus made was filled with common red size injection in a fluid state; not, however, quite so high as the surface of the coagulum. In cutting off a portion, the edge showed the coagulum to be very weak, much more so than it is commonly met with. The glass vessel was immediately put under the receiver of the air pump. Very early in the exhaustion, the carbonic acid gas was evolved in such quantity as to keep the fluid injection in a state of agitation, which had the advantage of keeping it fluid; when the exhaustion was increased, the evolvement was so rapid that it became necessary to work the pump very slowly. After the exhaustion had become nearly complete, the glass vessel and its contents were removed, and, with a view to fix and harden the coagulum, the glass vessel was placed in boiling water, which was renewed at short intervals, carefully preventing the water from coming in contact with the blood. This process melted the injection that had not passed into the coagulum, and allowed of its being poured off. The coagulum even now was by no means very firm, but capable of supporting itself; it was turned out upon a flat piece of glass. To make it dry more readily, and to prevent its going into

putrefaction, after having stood six hours, it was divided into slices half an inch thick. Forty-eight hours after it had been injected, I examined it with Mr. BAUER, and found its internal substance very minutely injected, there being only two small bursts of extravasation, each the size of a pea. Mr. BAUER's drawings of the appearance the injection put on immediately under the surface of the coagulum, and upon the cut edge of one of the sections, are annexed, and require little description, so perfectly do they represent the objects from which they were taken. I was assisted in this and the other experiments by Mr. CLIFT and Mr. GATCOMBE. This experiment decides the question respecting the structure of the net-work; since now the channels are filled with injection, their shape, size, and mode of ramification admit of every one examining them for himself; and one of the slices of the coagulum, in which this structure is seen, I have been able to preserve in spirit, and two others in oil of turpentine, so that the originals, as well as the drawings, are brought before the Society.* As the injection could only fill the spaces from which the carbonic acid gas was extracted, it cannot be doubted that the channels were formed by the gas.

Having brought these facts in proof of channels being formed in coagulated blood out of the body, and of their depending upon the evolvement of the carbonic acid gas contained in the blood, it next became necessary to ascertain whether coagula of blood deposited in the abdomen underwent the same change. To determine this point, I wounded one of the smaller branches of the mesenteric artery of a

* These specimens, two of injected coagula of venal blood, and one of arterial blood, are deposited in the Collection of the Royal College of Surgeons in Lincoln's-Inn-Fields.

rabbit, and the wound in the abdomen being closed, this small artery was allowed to bleed into the general cavity. In 48 hours the animal was pithed, and the arteries of the abdominal viscera were injected by the common minute injection coloured by vermillion. The cavity of the belly was then opened; it was in a perfectly natural state; there were no adhesions of parts; the small intestines were very vascular, and the vessels minutely injected. No serum was met with, and no large coagula of blood. One very small one was found lying upon the peritonæum in the right iliac region, and adhering to it in parts, but not by the whole surface of contact. This coagulum was evidently injected, and the only one I particularly examined, as there only remained one or two smaller coagula lying upon a portion of the intestinum ilium. All the rest of the blood that had been effused was absorbed. A drawing taken from this coagulum in the microscope, by Mr. Bauer, is annexed. He has shown the small arteries of the peritonæum entering the channels in the coagulum, which are larger than the arteries with which this communication has been formed; their appearance is also very different from that of arteries; they seem to have been over distended by the injection, and not to have acquired a regular form: there are three or four points of communication laterally between the channels in the coagulum and the arteries of the peritonæum, and it would appear that there is another communication immediately behind the centre of the coagulum. In all of these points, the smallness of the arteries, when compared with the channels themselves, is equally distinct. *Vide* Plate XII.

The appearance this injected coagulum put on, brought to

my recollection a preparation I had made in the year 1788, in which a small pedicle of coagulable lymph adhered to the surface of a portion of intestine, and had become vascular in less than twenty-nine hours, since the person died in that period after the operation for the strangulated hernia, and the intestine, when returned, had the natural polish on its surface. I had succeeded in injecting the arteries leading to it. An account of this case is published in the Appendix to my work upon Ulcers, and a drawing from the preparation is engraved in Mr. HUNTER'S work upon the Blood, Inflammation, and Gun-shot Wounds, but from not being magnified, the parts are indistinctly seen.

I requested Mr. BAUER to examine this preparation, which is preserved in the Hunterian Collection of Morbid Anatomy, and to make a magnified drawing of it upon nearly the same scale as that of the coagulum of blood, to show the difference, if there is any, between the appearance of the channels formed in exuded coagulable lymph, and extravasated blood. He has done so; and they correspond in the most essential particular, which is that the canal in the coagulum is larger in diameter than the artery by which it is supplied with blood. His drawing is annexed. Plate XIII.

By this means I have been enabled to present to the Society the appearance these channels put on, both in coagula of blood extravasated in the cavities of living animal bodies, and in exudations of coagulable lymph, at nearly the shortest possible periods in which they can be formed.

There is a preparation in the Hunterian Collection, of a coagulum of blood lying upon the testicle, of considerable size, which was produced by wounding an artery in tapping

a hydrocele; and from the circumstance of the testicle being extirpated a month afterwards, it was ascertained that the coagulum had remained there four weeks before the extirpation. The parts, immediately after the operation, were injected by Mr. HUNTER, and the coagulum was found to be vascular. This is the most satisfactory proof of coagulated blood having vessels Mr. HUNTER had met with. He has given several engravings of the coagulum, in his work on the Blood; but, from Mr. BELL not having Mr. BAUER's knowledge of drawing such objects in a magnified state, little information is to be received from an examination of the engravings. It occurred to me, that by a re-examination of this preparation, and putting a thin section of the coagulum, continuing the section into the substance of the testicle on which it lay, into the hands of Mr. BAUER, he might be able to make a magnified drawing of the parts, which would enable me to show, by comparing this drawing with the others, what changes are produced in the appearance of the channels, after they have for so long a time received the circulating blood; and whether the arteries by which they are supplied, had enlarged sufficiently to convert them into subordinate branches. The passages or channels originally formed by the extrication of the carbonic acid gas, were now found to have acquired a distinct coat, that admitted of being separated from the surrounding parts, showing them to be formed into regular tubes, but they still remained larger than the branches of the arteries by which they were supplied with blood, as will be seen by inspecting Plate XIV.

As the globules of pus are similar to those of the blood, I made experiments upon the fluid in which they are suspended,

and found inspissation produce the same effect on it, as coagulation does on the other; that a similar net-work is formed, and apparently by the same means, since if pus is deprived of its carbonic acid gas (of which it contains a large quantity) by exhaustion in the air pump, no such net-work takes place.

This is a fact of considerable importance in practical surgery, for since we now know that inspissated pus can become vascular, similar to coagulated blood, we have arrived at the principle on which granulations are formed, and from whence they derive the power of contraction, which is found to be inherent in them; we also can account for the great advantage of compression upon the surface of sores; since by that means all the superfluous pus is removed, leaving only enough for inspissation, in which state the carbonic acid gas is extricated, forming channels so as to admit of its becoming afterwards vascular, and then taking on the form of healthy granulations.

EXPLANATION OF THE PLATES.

PLATE VIII.

This plate contains six figures: in three of these the globules of the human blood are shown under different circumstances. In the three others are represented different views of the smallest fibre to which a muscle has been reduced in the field of the microscope.

Fig. 1. Shows 16 globules enveloped in their colouring matter, occupying, upon the micrometer, a superficies of 160,000 part of a square inch.

Fig. 2. Shows that when the colouring matter is removed, 25 globules occupy the same space.

Fig. 3. Shews that the globules, deprived of their colouring matter, and allowed to float in the coloured serum, when once in contact, appear to adhere in a manner not seen to take place when the globules are enveloped in the colouring matter.

These three figures are magnified 400 diameters, or 160,000 superficies.

Fig. 4. Muscular fibres from the thigh of a boiled chicken magnified 400 diameters, or 40,000 superficies.

Fig. 5. One single fibre.

Fig. 6. A fibre to shew a variety in its form.

The two last figures are magnified 400 diameters, or 460,000 superficies.

These fibres in their diameter correspond nearly with the globules of the human blood, deprived of their colouring matter.

Plate IX.

Represents the appearance which a drop of blood puts on immediately after it has been taken from the arm into a watch glass, and magnified 25 diameters, or 625 superficies.

That this appearance is produced by the carbonic acid gas, which is separated from the blood in the act of coagulation, is proved by no such appearance being met with when that gas has been previously removed by exhausting the blood in an air pump.

PLATE X.

Represents the same appearance magnified 200 diameters or 40,000 superficies, and shows that the globules of the blood take no part in forming it.

PLATE XI.

Represents a portion of coagulated blood injected under the receiver of the air pump, with the common minute injection, coloured by vermilion, by exhausting the coagulum of its carbonic acid gas, and the injection taking its place.

This plate contains two figures; Fig. 1 shows the course of the injection immediately under the pellicle which forms upon the surface of the coagulum: this is every where horizontal; for as the carbonic acid gas had no means of escape upwards, it was forced to move in an horizontal direction. Fig. 2 shows the injected part of the coagulum exposed in a section, rather oblique than vertical, in consequence of the coagulum being too soft to admit of the section being more direct. The drawing was made before a pellicle had formed upon the cut surface, which, had it taken place, would have rendered the surface opaque. The course of the injection is seen through the semi-transparent coagulum for some way into its substance. The round points are sections of portions of injection which had followed a horizontal course. The parts are magnified 12 diameters, or 144 superficies.

Plate XII.

Represents a small coagulum injected with coloured injection, from the arteries of the neighbouring parts. The coagulum was formed in consequence of an hæmorrhage from one of the smaller branches of the mesenteric artery, and was deposited upon the peritonæum, near the right groin, only 48 hours before death. It is worthy of observation, that the arteries through which the injection passed to the coagulum, are much smaller than the channels in the coagulum, and these channels are largest in the middle.

The coagulum extends through the whole breadth of the drawing, and the small arteries from the peritonæum open into its substance in several points. The parts are magnified 35 diameters, or 1225 superficies.

Plate XIII.

Represents a portion of human intestine, to which a small exudation of coagulable lymph is attached. This is injected from a small artery on the external surface of the intestine, less in diameter than the canal formed in the coagulum.

The opaque bodies lying upon the vein and close to the pendulous coagulum, are uninjected portions of coagulable lymph.

The parts are magnified 12 diameters, or 144 superficies.

Plate XIV.

This plate represents a section of a coagulum of blood that had remained in contact with that portion of the tunica vagi-

nalis which is attached to the body of the testicle, for a month before death, and had been afterwards injected with coloured minute injection from the spermatic artery.

The parts are magnified 85 diameters, or 1225 superficies.

The outline of the section of the body of the testicle on which the coagulum lies, and to which it is attached, is distinctly marked, and the exhalant arteries from the covering formed by the tunica vaginalis, are found to be very small, and in no proportion to any part of the tubes in the coagulum with which they have opened a communication.

The canals in this coagulum have acquired a regular shape, having undergone considerable changes since their first formation, which gives them a much nearer approach in appearance to the ramifications of arteries. They have acquired regular coats distinct from the parts by which they are surrounded.

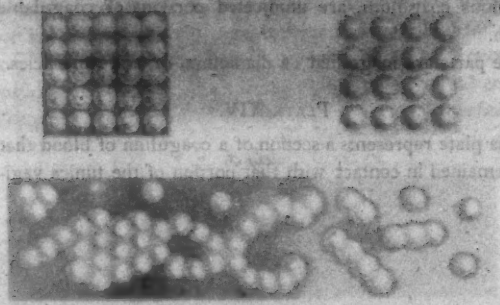


Fig. 5.

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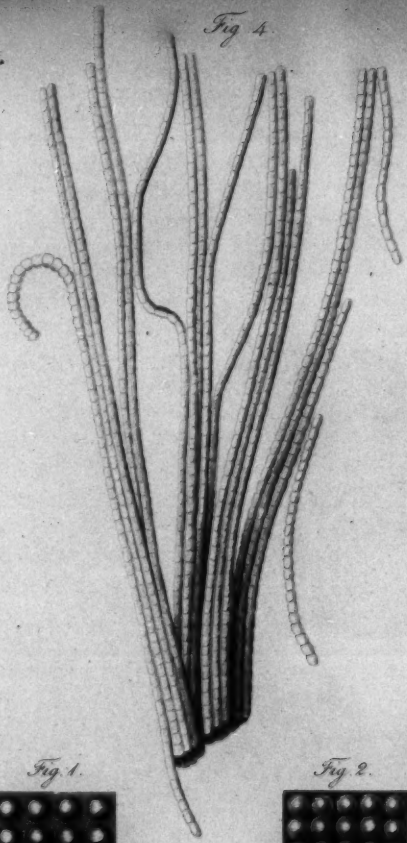

Fig. 6.

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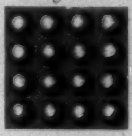

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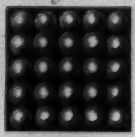

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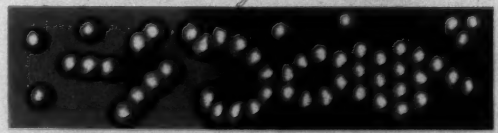

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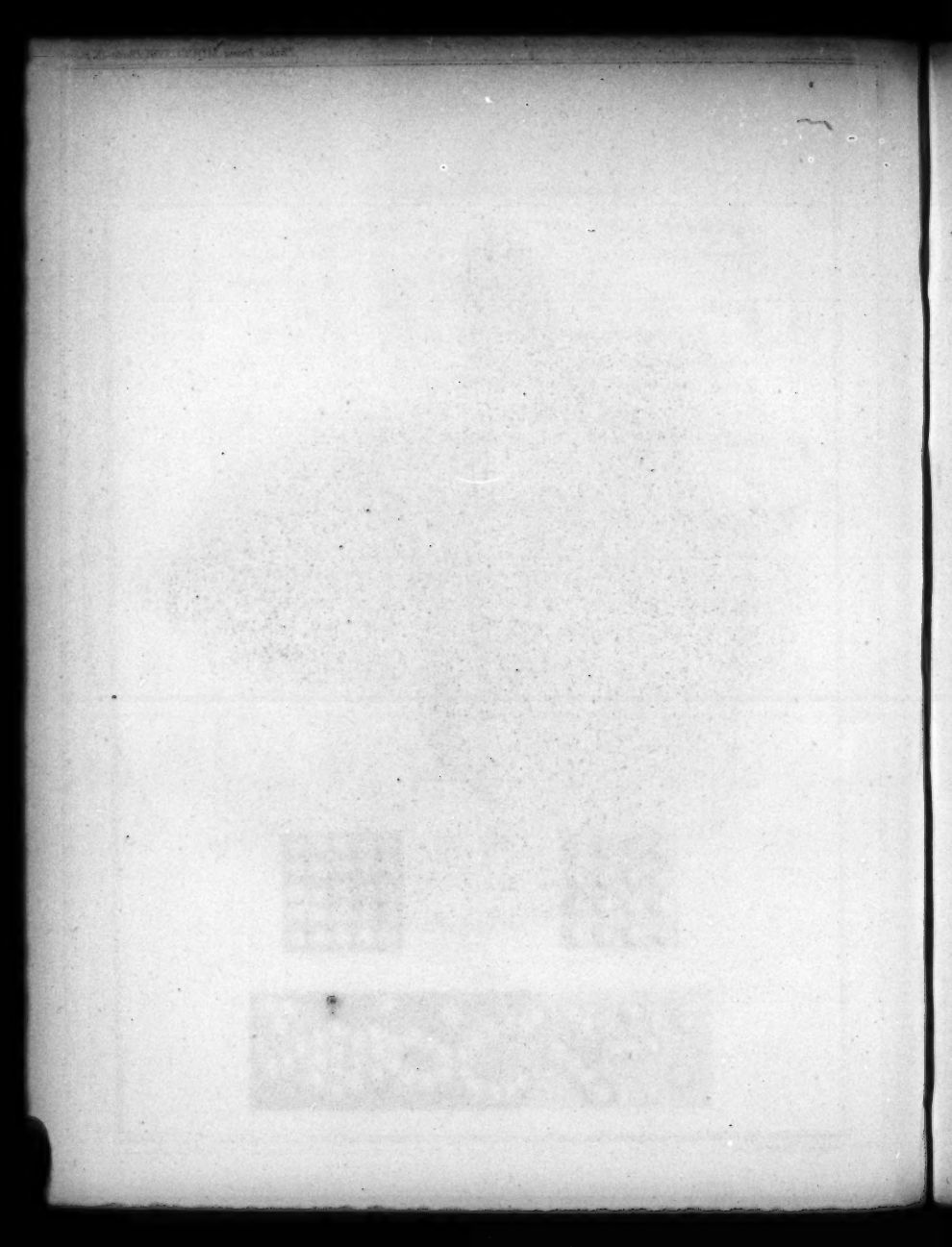

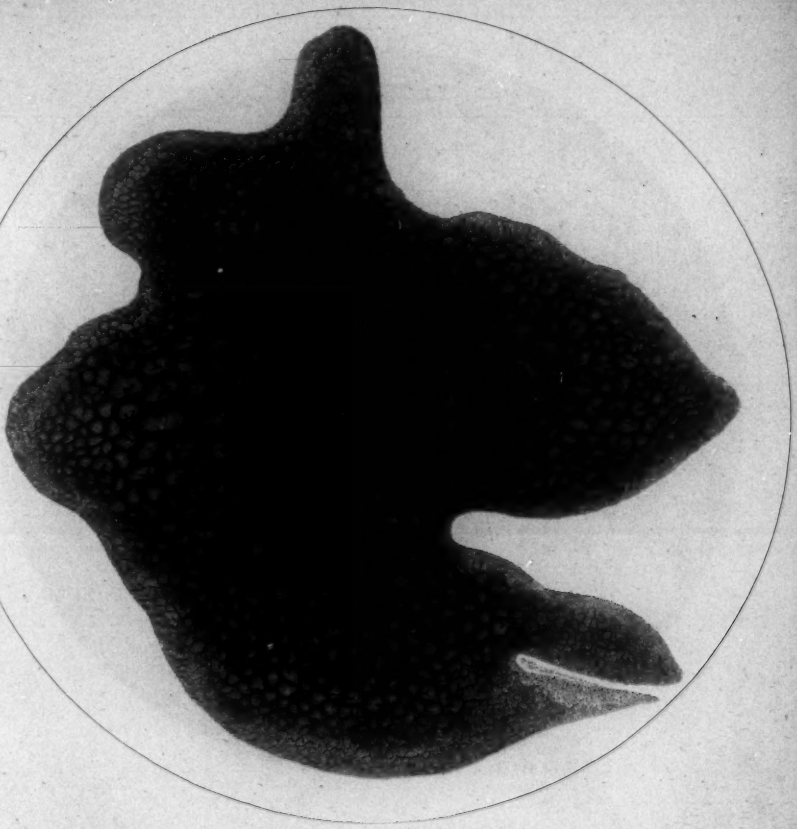

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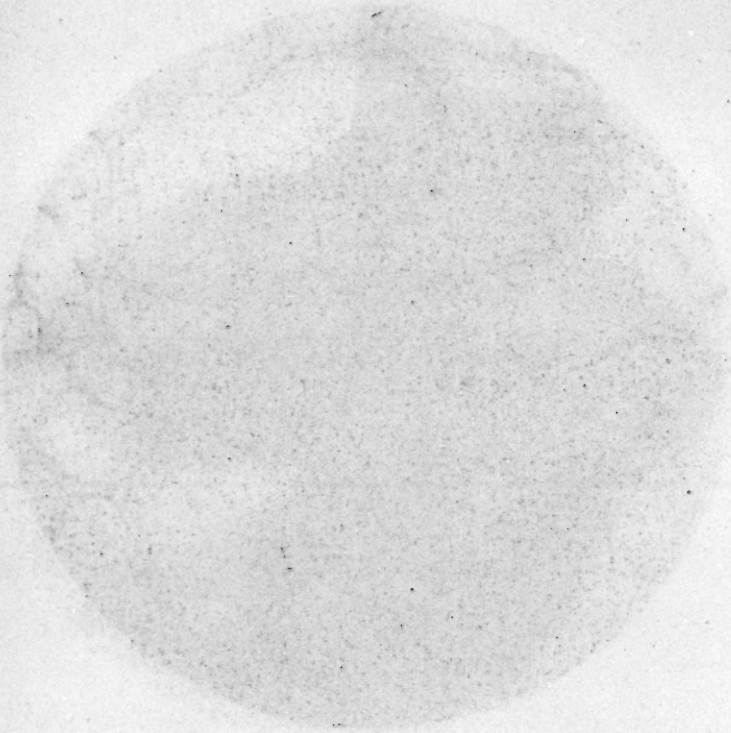

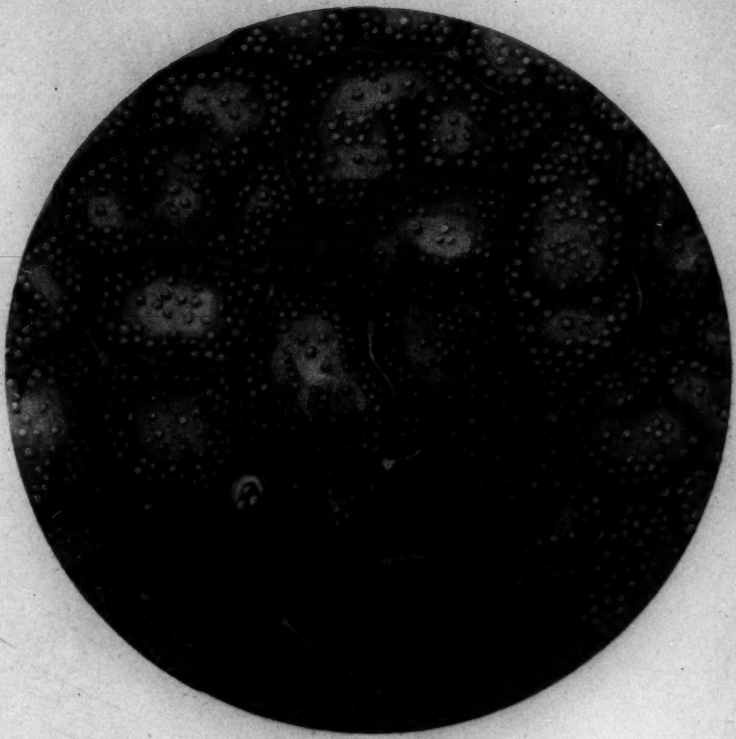

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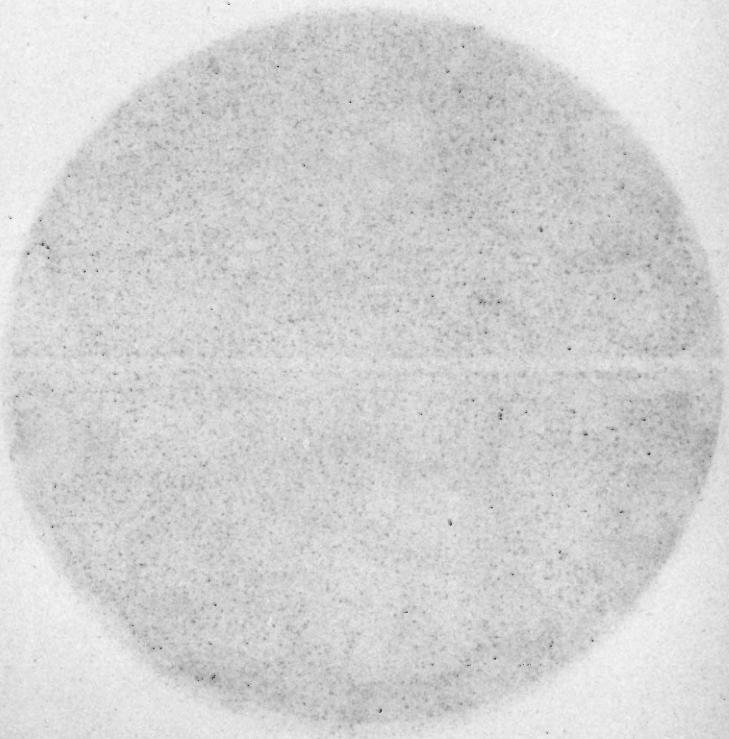

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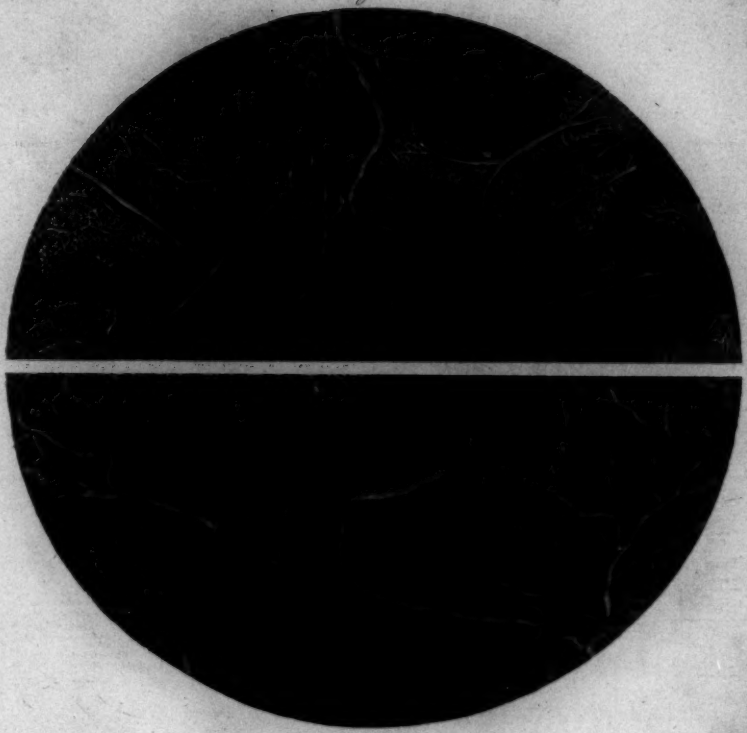

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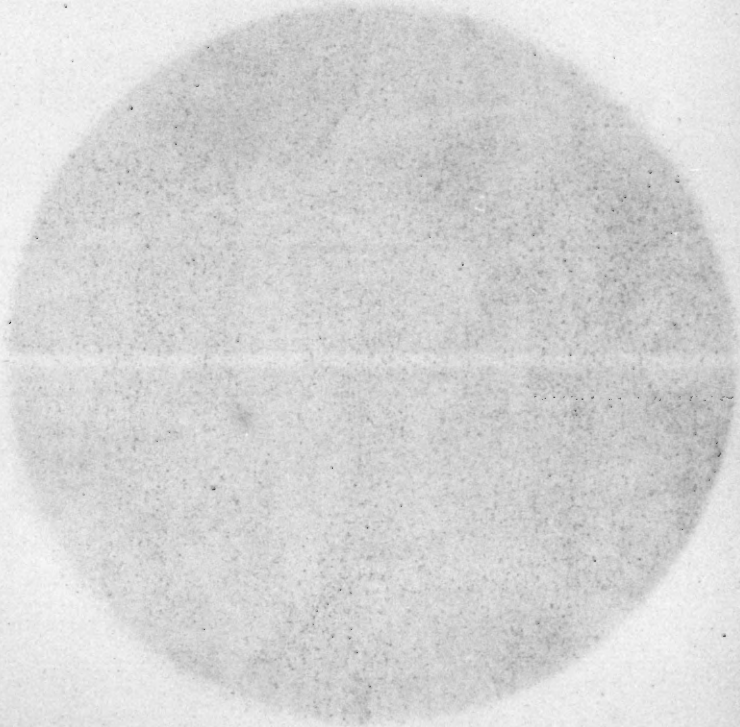

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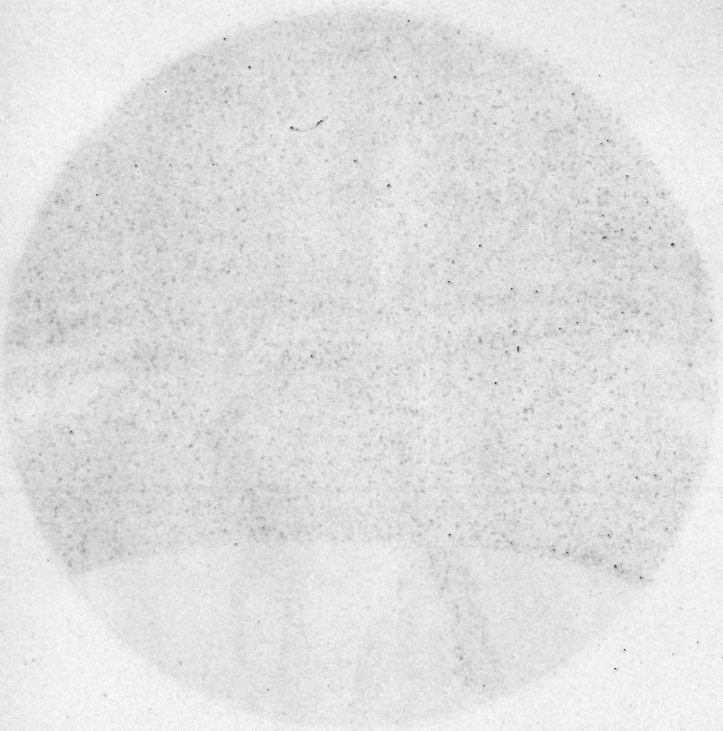

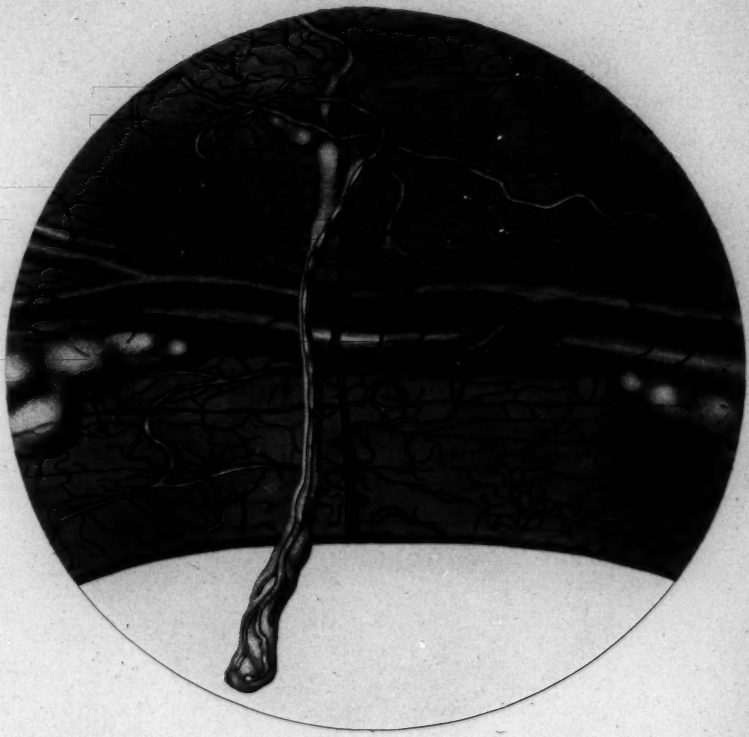

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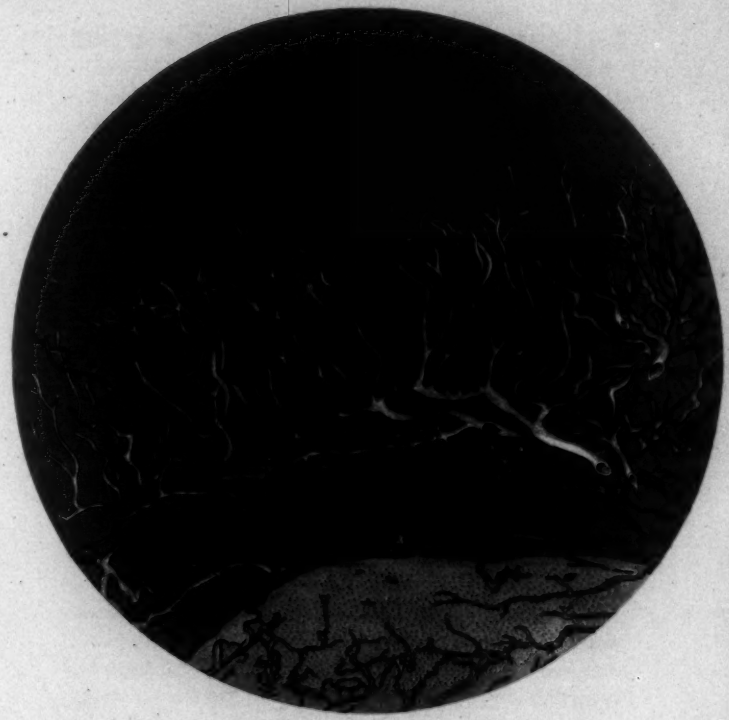


Franz. Baader del.

J.t Basire sc.

XIII. *On the laws of polarisation and double refraction in regularly crystallized bodies.* By David Brewster, *LL.D. F.R.S. Lond. and Edin. In a letter to the Right Hon. Sir* Joseph Banks, *Bart. G.C.B. P.R.S.*

Read January 15th, 1818.

Venlaw, June 1, 1817.

DEAR SIR,

In the different memoirs which you have done me the honour of submitting to the Royal Society, I have considered principally those branches of the polarisation of light which relate to the superficial action, or the superinduced properties of uncrystallized bodies. In the course of these enquiries, my attention was frequently directed to the phenomena of regular crystals; but from the difficulty of procuring proper specimens, and the extreme perplexity of the subject, it was not till lately that I succeeded in reducing under a general principle all the complex appearances which result from the combined action of more than one axis of double refraction.

Before I proceed to trace the steps which have conducted me to this general law, I must entreat the indulgence of the Society, while I attempt to give a brief and rapid view of the present state of our knowledge respecting the laws of double refraction. They will thus be able to appreciate more correctly the relative value of those successive generalisations by which this subject has been raised to one of the most interesting departments of physical science.

Dr. THOMAS YOUNG was the first person who directed the attention of philosophers to the perfect agreement between the beautiful theory of HUYGENS, and the double refraction of light as exhibited in calcareous spar. (see Phil. Trans. 1802, p. 45). The experiments of Dr. WOLLASTON afforded additional evidence of its exactness, and the more numerous and diversified observations of MALUS raised it to the rank of a general principle, which represented all the phenomena in the most accurate manner. About the same time, M. LAPLACE attempted to refer the deviation of the extraordinary ray to the action of those attractive and repulsive forces by which the ordinary refraction and reflection of light are produced. In this theory the aberration of the extraordinary ray is explained by a repulsive force emanating from the short diagonal of the rhomb of calcareous spar, or the axis of extraordinary refraction; and it is shown that the difference of the squares of the velocities of the ordinary and extraordinary ray is proportional to the square of the sine of the angle which this last ray forms with the axis; and that this difference represents that of the action of the crystal upon the two kinds of rays. LAPLACE then demonstrates that the principle of FERMAT, and the principle of least action lead to the law of HUYGENS, provided that in the principle of FERMAT the radius of the ellipsoid is taken to represent the velocity, while in the principle of least action it is made to represent the time in which light describes a certain space taken for unity.

This fine theory will no doubt be received by philosophers as a new proof of the high genius of its author; but while it is

thus received, we ought carefully to consider whether the HUYGENIAN law, upon which it rests, is the universal law of double refraction, or merely an elegant and correct expression of the individual phenomena of calcareous spar. Because this law represents with accuracy the action of a single crystal, is it thence to be presumed that all the other transparent crystals of the mineral kingdom possess the same structure, and exhibit the same series of appearances as calcareous spar? We venture to say that such a conclusion is scarcely possible in the present state of science; and that if it should happen to have been rashly drawn, it could not be deliberately supported by any one who is imbued with the cautious spirit of the inductive philosophy.

It will be replied, however, to this question, that MALUS has examined, with the utmost care, the properties of *quartz*, *arragonite*, and *sulphate of barytes*; that he has demonstrated the identity of their action with that of calcareous spar; and that the extension of the law of HUYGENS to other crystallized bodies can no longer be doubtful. This reply would have possessed considerable weight, had the alledged identity of action been satisfactorily established; but the experiments of MALUS are decidedly erroneous, for out of the *three* crystals, the identity of whose action with that of calcareous spar he is supposed to have demonstrated, no fewer than *two* have more than one axis of double refraction.

Here then is a difficulty of an unexpected kind: for if the experiments of MALUS on these crystals are set aside as incorrect, what confidence can we place in his observations on calcareous spar, upon which the truth of the HUYGENIAN law is founded? If nitre and arragonite, both of which have a

powerful double refraction, had, in place of calcareous spar, been put into the hands of HUYGENS, Dr. WOLLASTON, or MALUS, or of any other philosopher, however skilful, it is demonstrable that their measurements would have accorded with the theory of spheroidal undulations. LAPLACE would have connected this theory with the principles of mechanics, and it would have been universally received as a rigorously physical law. Yet after all this display of experimental and mathematical genius, the whole investigation would have turned out a delusion; for it can be shown, by decisive experiments, that both *nitre* and *arragonite* have two axes of double refraction; and that the aberration of the extraordinary ray cannot be explained by a single ellipsoid.

From these observations, therefore, we conceive it to be proved, not only that the HUYGENIAN law remains undemonstrated as the general law of double refraction; but that it remains undemonstrated as a correct expression of the individual phenomena of calcareous spar.

How then, it will be asked, are we to determine the law of extraordinary refraction, if the best experiments of our most eminent philosophers are set aside as insufficient? This can only be done in two ways, either by the discovery of new methods of magnifying and measuring with great exactness the deviation of the extraordinary ray, when the light passes near the axis of a crystal; or what is more practicable and correct, by examining the appearances which are exhibited by transmitting polarised light along the real or apparent axes of double refraction.

In the year 1811, M. ARAGO discovered, that when polarised light was transmitted through thin plates of sulphate

of lime and mica, and afterwards analysed with a prism of calcareous spar, they exhibited the most beautiful complementary colours. Without knowing what had been done by M. ARAGO, the same discovery was made by me in 1812; but though the general fact which each of us discovered was the same, yet I had the good fortune, by a peculiar mode of observation, to examine the phenomena seen along the real or apparent axes of crystals, and have thus been led to the results contained in the following paper,—results which, as will be seen in the sequel, could not possibly have been obtained by the mode of observation employed by M. ARAGO, and afterwards by M. BIOT.

The last of these philosophers has examined this class of phenomena with singular diligence and ingenuity. By a series of fine experiments, he has established many fundamental points in the science, and has associated his name with this branch of physics, as one of its most distinguished and successful cultivators. The crystals which M. BIOT examined were

Calcareous spar.	Tourmaline.	Sulphate of lime.
Rock crystal.	Feld-spar.	Sulphate of barytes.
Beryl.	Arragonite.	Sulphate of strontian.
Phosphate of lime.	Topaz.	Mica.

But his experiments for investigating the laws of moveable polarisation were principally made with *sulphate of lime*, on account of its perfect transparency, and the facility with which it can be separated into thin plates. The general result of these experiments is, that all the preceding crystals, with the single exception of certain specimens of mica, have

only one axis of double refraction from which the polarising force emanates; that all crystals are divisible into two classes, namely, attractive and repulsive; and that the laws investigated for sulphate of lime, abstracting the effects of certain secondary forces, are the general laws of polarisation for all other crystals.*

This view of the laws of polarisation, though deduced from ingenious investigations, and by methods which exhibit the finest talents for physical research, is incompatible with the experiments and observations contained in the following paper. No fewer than *seven* out of the *twelve* minerals employed by M. BIOT, have two or more axes of double refraction. Sulphate of lime itself belongs to this number; and all the irregularities of its action, which M. BIOT has represented by empirical formulæ, are the legitimate and calculable results of two rectangular axes. The division of crystals into attractive and repulsive, and the reference of the phenomena of mica to two repulsive axes, whose intensities are as 100 to 677, will also be found to be entirely hypothetical.

From this slight sketch of the present state of the science, it will be readily seen that the process of generalisation, however ingeniously it has been managed, has been carried on too rapidly, and has far outrun the progress of observation and experiment. In attempting, therefore, to establish new and more general laws, I began my researches by laying a broad foundation of facts. One hundred and sixty-five crystals were subjected to examination. In 165 of these, I have observed the property of double refraction. In about 100, I

* See BIOT's *Traité de Physique*, tom. iv. p. 377.

have been able to ascertain whether they have one or more axes; and by examining and measuring the tints which they exhibit at various angular distances from the axes whence the forces emanate, I have been led to a general principle which embraces all the phenomena, and extends to the most complex, as well as to the most simple developement of the polarising forces. This general principle is in no respect an empirical expression of the facts which it represents, nor is it supported by any empirical data. Founded on the principles of mechanics, it is a law rigorously physical, and enables us to calculate all the tints, &c. of the coloured rings, and all the phenomena of double refraction, with as much accuracy as we can compute the motions and positions of the heavenly bodies.

In the course of researches embracing the examination of such a great number of crystals, it was natural to expect that many new facts would present themselves that were but slightly connected with the general object of investigation. The phenomena of this kind, which I have had the good fortune to observe, comprehend several new properties of light, and of crystallized bodies, which I shall lay before the Society in a series of separate communications.

Section I. *On the crystals which produce double refraction.*

The property of double refraction was observed by the Abbè Hauy in *twenty* crystallized substances.* Malus has given a list of *nineteen* doubly refracting crystals, embracing

* *Traité de Mineralogie*, tom. i. p. 271, 272, Paris, 1801.

all the bodies in HAUY's table, except topaz, which he has accidentally omitted.*

The faculty of depolarisation, which I have explained in a former paper,† has been considered as a sufficient indication of the existence of two separate images; and upon this principle it has been stated, that all crystals have the property of double refraction, whose primitive form is neither the cube nor the regular octohedron.‡ This statement, however, is by no means correct, for the rhomboido-dodecahedral crystals ought also to have been excepted; and I have besides shown, by numerous experiments, that almost all the crystals which have these forms, exhibit an imperfect doubly refracting structure, which some of them possess to a very considerable degree. But admitting the statement to be unexceptionable, it could never have been used as a rule for determining whether a crystal refracts doubly or singly; for it is much more difficult to detect the primitive form of a crystal, than to examine, by direct experiments, its optical properties. Tungstate of lime, for example, would have been reckoned a crystal without double refraction, when HAUY believed its primitive form to be the cube, although it possesses this property in a very high degree.

In order, therefore, to determine whether crystals have the property of double or of single refraction, we must ascertain by direct observation, if they form two images, or possess the property of regular depolarisation. In this way I have obtained the results in the following table; but as the exist-

* *Theorie de la Double Refraction*, p. 96, Paris, 1810. † *Phil. Trans.* 1815, p. 27.
‡ *Edin. Transactions*, vol. viii. Part. I. BIOT's *Traité de Physique*, tom. iii. p. 325.

ence of two separate images is the only correct and infallible test of double refraction, I have employed it for the greater part of the substances in the table. When a crystal possesses the property of depolarising light, it only proves that it forms two pencils polarised in opposite planes; though there can be little doubt that one of them is subject to the extraordinary law of refraction.

Table of doubly refracting crystals:

Chromate of lead
Carbonate of lead
Tungstate of lime
Carbonate of potash
Nitrate of potash
Calcareous spar
Arragonite
Zircon
Sulphur
Acetate of lead
Acetate of nickel
Ruby
Sapphire
Corundum
Beryl
Emerald
Cymophane
Peridot
Epidote
Euclase
Topaz
Pycnite
Tourmaline
Rubellite
Quartz
Agate
Dichroite
Bitter spar
Apatite
Idocrase
Mica
Lepidolite
Talc
Indurated talc
Chlorite
Kyanite
Stilbite
Cubizite
Apophyllite
Prehnite
Hydrate of magnesia
——— barytes
——— strontites
Feldspar

Mellite
Realgar
Native orpiment
Ruby silver
Actynolite
Anhydrite
Litharge
Cinnabar
Tincal, or native borax
Borate of soda
Mesotype
Sphene
Harmotome
Macle
Nepheline
Augite
Datolit
Wavellite
Calamine
Anthophyllite
Laumonite
Titanite
Asbestos
Serpentine
Steatite
Diopside
Staurotide
Diallage
Hyacinth
Tremolite
Semiopal
Spodumene
Tabular spar
Benzoic acid
Oxalic acid
Citric acid
Tartaric acid
Boracic acid
Phosphoric acid
Succinic acid
Chromic acid
Cryolite
Benzoate of ammonia
Sulphate of cobalt
——— lead
——— iron
——— zinc
——— copper
——— copper and iron
——— nickel
——— barytes
——— strontites
——— lime
——— magnesia
——— potash
——— soda
——— ammonia
——— magnesia and soda
Carbonate of copper
——— barytes
——— strontites

Carbonate of soda
——— ammonia
——— copper
Muriate of mercury
——— gold
——— silver
——— copper
——— iron
——— lead
——— magnesia
——— lime
——— barytes
——— strontites
Nitrate of silver
——— copper
——— zinc
——— bismuth
——— mercury
——— ammonia
——— lime
——— strontites
——— soda
——— magnesia and ammonia
Acetate super of copper
——— super of copper and lime
——— zinc
——— soda
——— potash
Phosphate of iron
——— copper
——— lead
——— soda
——— sub of potash
Arseniate of lead
——— iron
——— copper
——— potash
Tartrate of potash
——— potash and soda
——— super of potash
——— potash and antimony
Oxalate of ammonia
Superoxalate of potash
Prussiate of potash
Hyperoxymuriate of potash
Oxymuriate of mercury
Calomel
Mother of pearl
Ice
Camphor
Sugar
Crystallized Cheltenham salts
Murio-sulphate of iron and magnesia
Emerald copper

160 Muriate of lead
Sulphate of ammonia and magnesia
——— red oxide of manganese
——— alumine
Specular iron
165 Pargasite

SECTION II. *On crystals with one apparent axis of polarisation.*

Having thus distinguished the crystals which possess double refraction, from those which are destitute of this property, we shall now proceed to determine their optical structure, or to ascertain whether the forces which act upon the extraordinary ray emanate from one or more axes.

If we transmit polarised light through any of the parallel surfaces of the primitive hexaedral prism of a crystal of beryl, and analyse the emergent pencil by a prism of calcareous spar, having its principal section placed either parallel or perpendicular to the plane of primitive polarisation, it will be found that, when the axes of the prism of beryl is inclined 45° to the plane of primitive polarisation, the vanished image is restored. If the thickness of the crystal exceeds 0.035 of an inch, the restored light will be nearly white.

But if the polarised light is transmitted along the axis of the prism of beryl, there will be seen a series of beautiful circular concentric rings whose centre is the intersection of the arms of a dark rectangular cross, and containing all the tints in NEWTON's table of the colours of thin plates. These rings, which are shown in Plate xv. fig. 1, increase in diameter as the length of the prism is diminished; but they may be distinctly seen, even in the longest prism of beryl. If we now examine the double refraction of beryl by transmitting

the common light through two surfaces very much inclined, it will be seen that the separation of the two images is a *maximum*, when the transmitted ray is perpendicular to the axis of the prism, and that the two images gradually coalesce into one, as the transmitted ray becomes parallel to the axis. The axis of the prism, therefore, is the apparent axis of double refraction, and likewise the axis of the system of rings shown in fig. 1. Hence we conclude that beryl has one apparent axis of extraordinary refraction, and one apparent axis of polarisation which are coincident. The same property of producing the rings in fig. 1, I have discovered in the following crystals.

A list of crystals with one apparent axis of double refraction and polarisation.

Negative class.

- Beryl, emerald
- Calcareous spar, bitter spar
- Sapphire, ruby
- Rubellite, tourmaline
- 5 Phosphate of lime
- Hydrate of magnesia
- ——— strontites
- Idocrase
- Mellite
- 10 Nepheline
- Arseniate of copper
- Potash
- Muriate of lime

Negative class.

- Muriate of strontian
- 15 Apophyllite
- Nitrate of soda
- Subphosphate of potash
- Sulphate of nickel (certain specimens of)

Positive class.

- Zircon
- 20 Quartz
- Ice
- Super acetate of copper and lime
- Sulphate of potash (certain specimens of)

As several of the crystals contained in the preceding list are not capable of being cut into transparent prisms with large refracting angles, it is impossible to ascertain the relation between the force of double refraction, and the polarising

force, by the action of which the coloured rings are produced; but, in all those crystals upon which this experiment can be made, I have invariably found that the two axes from which these forces emanate, are coincident, and that the force which produces the deviation of the extraordinary ray increases and diminishes with the polarising force which produces the coloured rings.*

In proceeding to examine the nature and properties of the coloured rings produced by the different crystals in the table, we shall first consider the effect produced by plates of different thicknesses. If we take a rhomboid, of calcareous spar, whose principal section is A B C D, fig. 2. and cement upon its two surfaces AB,CD, two prisms of flint glass, having their sides perpendicular to the principal section, and their refracting angles EBF, GDH, a little greater than FB*b* which is 45° 23′ 26″; and if polarised light is incident, perpendicularly upon the surfaces BE,DG, it will be transmitted parallel to B*b*,† and will exhibit in the most beautiful manner the system of coloured rings shown in

* M. Biot was the first who deduced this conclusion for crystals with one axis from experiments on calcareous spar and rock crystal; but it could not be considered as a general fact when drawn from such a small number of crystals. It will be seen in the sequel of this paper, that I have extended the result to crystals with two axes, and have thus established it as a general principle.

† This mode of exhibiting the coloured rings, is greatly superior to the mode employed by Biot, of grinding down the solid angles at B and D. Beside the advantage of procuring the greatest possible thickness from a given rhomboid, we preserve the polish of its natural faces, and the thickness of crystal through which the polarised ray passes can be calculated with the utmost accuracy from the thickness B*b* of the plate. This method is also peculiarly favourable for showing the influence of pressure upon the polarising structure of the crystal, and for various other experiments on the coloured rings.

fig. 1.* Let the rhomboid ABCD be now cut into two plates, by any line MN, and let the rings, produced by each of the plates, be examined separately in the way already described: it will then be found that the squares of the diameters of the rings are in every case proportional to the numbers which represent the corresponding tints in NEWTON's table; and that the squares of the diameters of similar rings, as produced by plates of different thicknesses, are reciprocally proportional to the square roots of these thicknesses. These two results, which were first obtained by M. BIOT, lead to the general conclusion that the tints produced at different inclinations to the axis of the crystal, are to one another, as the square of the sine of the angle which the polarised ray forms with that axis. This law of the tints for crystals with one axis, was deduced by M. BIOT from the action of rock crystal, as well as calcareous spar; and also from the phenomena of certain specimens of mica which he supposes to have one axis. I have found it perfectly correct in all the other crystals contained in the table; and it may therefore be considered as a general law, which we may apply with confidence in our future researches.

* The system of coloured rings produced by one axis of double refraction, and the still more beautiful and complicated system produced by two axes, were discovered by me in the year 1813. I observed the former in *beryl*, *emerald*, *ruby*, &c. and the latter in *topaz*, *mica*, and a great variety of other minerals. Dr. WOLLASTON was the first who detected the circular system of rings in *Iceland spar*, and they were shown to me by that eminent philosopher in July 1814. In a letter dated Dec. 3, 1815, M. BIOT announced to me, that he had then discovered the circular rings in Iceland spar, and it appears that the same observation was made by M. SEEBECK, in Dec. 1815; these dates, however, are nearly a year and a half posterior to that of Dr. WOLLASTON's experiment.

In order to apply it however with the utmost simplicity, I have found it very convenient to consider every crystal as cut into a sphere, one of whose diameters is the axis of double refraction, and to suppose that the polarised ray passes through the centre of the sphere. By this means we have no occasion to consider the refractive power of the crystal, or the difference of thickness arising from oblique transmission; for the polarised ray is always incident perpendicularly, and the thickness of the sphere is every where the same. If in this sphere, AB, fig. 3. is the axis of the system of rings, then P*p* may be called the *diameter of no polarisation*, or the *apparent axis of double refraction and polarisation*; P and *p* the *poles of no polarisation*; COD, the *equator of maximum polarisation*, *and* EF the *isochromatic lines* or *curves of equal tint*. Now, since the tint varies as the square of the sine of the angle which the transmitted ray forms with the axis P*p*, it will be a *maximum* in every point of the equator COD, and will be represented by Sin.^{2}. 90°. Hence if ϕ be the angle which any other diameter EO forms with the axis, the tint at E, and at every point of the parallel EF, will be represented by Sin.^{2} ϕ. By determining therefore experimentally the tint t, produced at any given thickness B, and at any inclination ϕ, the maximum tint T, for that thickness, will be $T = \frac{t}{\text{Sin.}^2\phi}$ and the tint for any other thickness b will be $T = \frac{b}{B} \times \frac{t}{\text{Sin.}^2\phi}$. It is obvious from these formulæ, that any given tint can be developed at any angle or distance from the axis, merely by varying the thickness of the crystal or the diameter of the sphere.

If it should be required to find the tints corresponding to any angle of incidence upon the natural faces of the crystal,

we have only to consider the position of our sphere within the crystal, and compute the inclination of the refracted ray to the axis. Thus in calcareous spar the axis P*p* corresponds with the short diagonal AB of the primitive rhomb, as shown in fig. 8. In *beryl*, *zircon*, *sapphire*, *rubellite*, *rock crystal*, *nepheline*, *idocrase*, *muriate of lime*, *super acetate of copper and lime*, *muriate of strontian*, &c. it corresponds with the axis of the prism in which they generally crystallize; while in *hydrate of magnesia*, *arseniate of copper*, &c. it is perpendicular to the laminæ of which they are composed.

Although the systems of rings produced by all crystals with one axis, exhibit the same tints, and possess the same properties, yet those which are produced by the five crystals of the negative class, namely, *zircon*, *quartz*, *ice*, *super acetate of copper and lime*, and certain specimens of *sulphate of potash*, will be found to differ in one essential particular from the system produced by all the other crystals. If we take two rhomboids of calcareous spar and place the one symmetrically on the other, the system of rings which they exhibit will be exactly the same as would have been produced by one rhomboid whose thickness is equal to the sum of the thicknesses of the two which are combined. M. BIOT has deduced this result from theory, and attempted in vain to confirm it by experiment;* but from the peculiarity of the method of observing

* J'ai employé ainsi conjointement les deux plaques de spath d'Islande dont j'ai donné plus haut les épaisseurs; et j'ai eu en effet des anneaux bien plus petits que par une seule d'entre elles; mais la difficulté d'aligner également les axes de ces deux plaques m'a empêché de mettre dans les expériences l'exactitude nécessaire pour les mesurer. J'aurais voulu aussi combiner des cristaux attractifs avec des cristaux repulsifs, mais je n'en ai pas eu l'occasion. BIOT *Traité de Physique*, tom. iv.

the rings shown in fig. 2, I have been able to prove it by direct observation. The exact parallelism of the sides of the two rhomboids, when their surfaces are not perfectly flat, can easily be obtained by separating them with a piece of soft wax, and observing the perfect coincidence of the reflected images from the two adjacent surfaces; or by looking at the system of rings, and altering the position of the crystals till they become quite perfect. I have obtained a similar result by combining a plate of *beryl* with a plate of *calcareous spar*, for the system of rings will always be the same as would have been produced by two plates of beryl, one of which was the plate employed, and the other a plate which gave rings of the same size as the plate of calcareous spar.

But when we combine the system of rings produced by a crystal of *zircon*, &c. with the system produced by *calcareous spar*, a very different effect is produced. The system of rings instead of being diminished is increased, and is equal to the system which would have been produced by a thin plate of calcareous spar, whose thickness is equal to the difference of the thicknesses of the plate of calcareous spar employed, and another plate of calcareous spar, that would give rings of the same size as those given by the zircon, &c. alone. This result, which I succeeded after much labour in obtaining experimentally, will also be obtained by substituting ice in place of zircon. Quartz cannot be employed in these experiments, as the system of rings is never complete, on account of the secondary tints which M. BIOT discovered along its axis. If the plate of zircon, &c. gives a system of rings of the very same size as those of calcareous spar, the one system will be com-

pletely obliterated by the other, and the combined crystals would exhibit neither double refraction nor polarisation. Hence it follows that *zircon, quartz, and ice, superacetate of copper and lime*, and certain specimens of *sulphate of potash*, form a class separate from all the other crystals in our table; and that the polarising force in the one class, is either of an opposite nature, or exerted in an opposite direction to the polarising force in the other class. This opposition in the action of crystals was first observed by M. BIOT, in plates cut obliquely to the axis; and by this mode of observation he divided the crystals which he examined, into two classes, in the following manner:

REPULSIVE CLASS.	ATTRACTIVE CLASS.
Calcareous spar	Rock crystal
Arragonite	*Sulphate of lime*
Beryl	*Sulphate of barytes*
Tourmaline	*Topaz*

But since the crystals marked in italics have two or more axes of double refraction, and cannot, as we shall afterwards show, be ranked in either class, M. BIOT's list, when corrected, will stand thus:

REPULSIVE CLASS.	ATTRACTIVE CLASS.
Calcareous spar	Rock crystal
Beryl	
Tourmaline	

agreeing, in so far as it goes, with the more extended table which we have already given. Hence it is obvious that M. BIOT could not have made the experiment which he proposes to make, at the end of the passage just quoted; namely, to

combine the system of rings formed by the attractive crystals, with the system formed by the repulsive ones; for rock crystal being the only attractive substance of one axis with which he was acquainted, was quite unfit for this purpose, from the imperfection of the system of rings which it produces. The discovery of the optical structure of *zircon** has enabled me to perform the experiment with success.

This opposition of structure between *quartz* and the other four crystals with one axis, contained in M. BIOT's table, is ascribed by this eminent philosopher to an opposition in the forces which act upon the extraordinary ray; and he considers it as an established point, that in rock crystal, the extraordinary ray is attracted to the axis, while in the other crystals it is repelled from it. Hence he has concluded, that the phenomena may be represented in the first case by a prolate ellipsoid, and in the second case by an oblate ellipsoid as HUYGENS had already shown; and has thus been led to divide crystals into two great classes, *attractive* and *repulsive*. This division, however, is entirely hypothetical, in so far as the polarising forces is concerned, for it will afterwards be shown that the phenomena of rock crystal may be explained by a negative force emanating from two equal rectangular axes, or that the phenomena of the other class may be explained by a positive force in a similar manner; or if we prefer the analogies which the sciences of magnetism and

* The extreme difficulty which attends experiments of this kind, will be understood from the fact, that I cut more than fifteen plates out of a large piece of *zircon* without discovering its axis. By a singular accident, however, Mr. MORTON, jeweller, in Edinburgh, procured for me no fewer than sixty plates of zircon with parallel faces, and it was only in two of these that the system of rings was developed.

electricity present, we may explain the phenomena of both classes, by supposing each crystal to be endowed both with positive and negative axes.

In marking, therefore, this difference of action, I have employed the terms *positive* and *negative*, as denoting merely the *opposition*, and not the *nature* of the polarising forces.

Hitherto we have considered the system of coloured rings as produced only near the axes of crystals, or as capable of being developed at any distance from the axis, merely by diminishing the size of the sphere; but there are two modes by which the rings can be rendered visible at any distance from the axis, and with any thickness of crystals.

If we take a prism of flint glass, with a large refracting angle, and examine through it the system of rings, we shall find that instead of nine or ten rings, which are visible without the prism, we may reckon, by estimation, from 80 to 100 on that side of the axis towards which the refraction is made. This observation is analogous to that of NEWTON upon the rings formed by thin plates, and decidedly proves that the coloured tints are actually produced at distances from the axis where the phenomena of fixed polarisation are exhibited.

The other mode of developing the rings at any distance from the axis, and with any thickness of crystal, consists in crossing the tints with plates of *rock crystal* cut parallel to its axis of double refraction, or with laminæ of *sulphate of lime*. If the axis AB, Pl. XV. fig. 1, of a plate of rock crystal, having such a thickness as to produce no coloured tints, even at a considerable obliquity, is placed in the direction of a diameter of the rings given by calcareous spar, a new system of rings will be produced at A and B, having their centres at O. The

point where A,B, crosses the middle ring will be black, as the action of the calcareous spar is there exactly counterbalanced by the action of the rock crystal. The rings within CD, EF will be *positive*, in consequence of the action of the rock crystal predominating, and those without them will be *negative*, in consequence of the action of the calcareous spar predominating. If the plate of *rock crystal* or *sulphate of lime* produces the rings at *a*, *b*, instead of A,B, then in the other two quadrants, *c*, *d*, the tints will be those which are produced by the sum of the actions of the crystals, and the tints at *c* and *d* will be double of that which is produced at these points by the calcareous spar alone.*

One of the most interesting features of the system of coloured rings, is the black cross MN, PR, where the two arms are exactly at right angles to each other. If the plate which produces this system of rings is turned round its axis O, the rings and the black cross will preserve an invariable

* The developement of colours by the opposite action of two crystallized plates, which are not capable of producing them separately, was first effected by M. BIOT, before the discovery of the coloured rings, and by the aid of his divided apparatus; but the preceding method of developing the rings by these opposite actions, is more general, and is so extremely simple, that it can be easily done by any person, merely by holding the crystals in his hands. The difficulties experienced by M. BIOT are thus stated by himself: "Mais telle est l'exactitude qu'il faut mettre à ces recherches que mes premières tentatives n'eurent aucun succès, parce que je m'étais borné à opérer le croisement à la main au lieu d'employer les appareils divisés qui m'avaient servi jusqu'alors. Car la grande énergie du spath d'Islande fait que si l'on s'écarte le moins du monde des positions indiquées par la théorie, on passe à la polarisation totale et les couleurs ne peuvent plus se developper. Cette cause faisait aussi qu'il était plus difficile de tomber précisément sur la proportion des épaisseurs de chaux sulfatée qui pouvait servir à compenser les lames de cristal d'Islande soumises à l'expérience." *Mém. de l'Institut*, Lû 27 Dec. 1813.

position, one of the arms, MN, being always in the plane of primitive polarisation, and the other, PR, in a plane perpendicular to it. Hence it follows, that whenever a plane passing through the axis of the crystal, is either in the plane of primitive polarisation, or perpendicular to it, the extraordinary pencil or tint disappears. This result will be found correct, whatever be the direction in which the polarised ray traverses the crystal, and is therefore the general law of the disappearance of the extraordinary pencil for crystals with one axis.

SECTION III. *On crystals with two or more axes of polarisation.*

The complicated structure of mica was first discovered in 1812 by M. BIOT and myself, without any knowledge of each others observations. M. BIOT considered one kind of mica as the only mineral known to possess this compound structure, indicating the existence of two axes; whereas, so early as the beginning of 1813, I had found the same structure in *topaz*,* *nitre*, *sulphate of potash*, *tartrate of potash and soda*, *acetate of lead*, and *mother of pearl*; and I have since discovered that this is the general structure of doubly refracting crystals.

The following is a list of crystals, which I have found to possess more than one axis of polarisation and double refraction.

Arragonite	5 Stilbite
Mica	Cymophane
Topaz	Axinite
Feldspar	Olivine

* The drawings of the rings in *topaz*, which I published in the Phil. Trans. for 1814, are an accurate representation of the phenomena, very near the resultant axes of crystals with more than one axis.

Epidote
Kyanite
Talc
Prehnite
Dichroite
Lepidolite
Mother of pearl
Indurated talc
Sulphate of lime
——— barytes
——— strontian
——— lead
——— magnesia
——— iron
——— copper
——— copper and iron
——— ammonia
——— potash
——— soda
——— zinc
——— cobalt
——— ammonia and magnesia
——— soda and magnesia
——— nickel
——— manganese
Carbonate of lead
——— soda
——— ammonia
——— potash
Nitrate of silver
——— ammonia
——— potash
——— lime
——— bismuth
——— mercury
——— zinc
——— strontian
Nitrate of copper
Borax of the shops
Tartaric acid
Citric acid
Hyper-oxymuriate of potash
Phosphate of soda
Prussiate of potash
Tartrate of potash and soda
Oxalate of ammonia
Super-oxalate of potash
Crystallized Cheltenham salts
Rhomboidal salt
Murio-sulphate of magnesia and iron
Benzoate of ammonia
Mesotype
Oxalic acid
Chromic acid
Muriate of barytes
——— mercury
——— magnesia
——— copper
Sulphur
Hydrate of barytes
Super-tartrate of potash
Tartrate of potash
——— potash and antimony
Boracic acid
Succinic acid
Chromate of lead
Diallage
Spodümene
Tincal or native borax
Anhydrite
Sugar
Acetate of lead
——— copper

By examining the preceding list, and comparing it with the list of crystals that have one axis, we may conclude,

1st, that the combinations of sulphuric and tartaric acid with earthy, alkaline, and metallic bases, have two or more axes of double refraction.

2nd, that as only about 23 crystals have one apparent axis of double refraction, while more than 80 have two axes, and as the phenomena of these 23 crystals can be referred to two axes, as will afterwards be shown, the general laws of polarisation and double refraction remain yet to be investigated by experiment.

If we compare the minerals in the preceding list with the most recent table of primitive forms,* it will be found that there is a connection by no means equivocal between the primitive form of a crystal and the number of its axes of extraordinary refraction. In order to establish this curious result, I have drawn up the following Table, in which I have inserted the names of the minerals and the number of their axes opposite to the different primitive forms under which they have been classed.

THREE AXES.	Muriate of soda Boracite Leucite Analcime Aplome Phosphate of manganese and iron	Cube.
TWO AXES.	Muriate of barytes Sulphate of magnesia Prussiate of potash Mesotype Sulphate of nickel ——— zinc	Right quadrangular prism with a square base.

* The table of primitive forms which I have used, is the one drawn up by Dr. THOMSON in the Article *Crystallography*, in the Edinburgh Encyclopædia, as it contains many of the observations made by HAUY and BOURNON, since the publication of HAUY's *Traité de Mineralogie*.

Two Axes.	Anhydrite, Cymophane, Prehnite, Peridot, Stilbite, Tartrate of potash	Right quadrangular prism with a rectangular base.
Two Axes.	Sulphate of strontian, Sulphate of barytes, Mica, Talc, Spodumene, Sulphate of soda, Carbonate of potash, Tartrate of potash and soda, Citric acid	Right quadrangular prism; base a rhomb.
Two Axes.	Sulphate of lime	Right quadrangular prism;
	Epidote	base an oblique parallelogram.
	Axinite	
Two Axes.	Borax	Oblique quadrangular prism with a rectangular base.
Two Axes.	Oxalic acid, Sugar, Sulphur, Acetate of copper, Tartaric acid, Chromate of lead	Oblique quadrangular prism; base a rhomb.
Two Axes.	Feldspar, Kyanite, Sulphate of copper	Oblique quadrangular prism; base an oblique parallelogram.
One Axis.	Calcareous spar, Bitter spar, Quartz, Tourmaline	Rhomboid with an obtuse summit.
Two Axes.	Sulphate of iron	Rhomboid with an acute summit.
One Axis.	Arseniate of copper, Apatite, Beryl, Emerald, Nepheline, Sapphire, Ruby	Regular hexedral prism.
Three Axes.	Garnet, Blende	Rhomboidal dodecahedron.
One & Two Axes.	Sulphate of potash	Bipyramidal dodecahedron.
Three Axes.	Fluor spar, Muriate of ammonia, Alum, Spinelle ruby, Pleonaste, Diamond, Ruby copper, Nitrate of lead	Regular Octohedron.
Two Axes.	Nitrate of potash, Sulphate of soda, Arragonite*, Topas, Carbonate of lead†, Sulphate of lead	Octohedron in which the pyramids have a rectangular base
One Axis.	Zircon, Mellite, Idocrase, Apophyllite, Arseniate of potash	Octohedron in which the pyramids have a square base.
Two Axes.	Carbonate of soda	Octohedron in which the pyramids have a rhomb for their base.

* BOURNON makes it a quadrangular rectangular prism with a square base.

† BOURNON makes it a quadrangular prism with a rhomboidal base.

It appears from the slightest examination of the preceding Table, that all the crystals with *one*, *two*, or *three* axes, range themselves under their particular primitive forms, and that the only exceptions to this general fact, are *Idocrase* and *Apophyllite*, which we have no doubt will be found to have primitive forms, different from those assigned to them by HAUY. All the crystals with one axis have for their primitive forms a hexaedral prism, a rhomboid with an obtuse summit, and an octohedron in which the pyramids have a square base. All the crystals with three axes, have the cube, the regular octohedron, and the rhomboidal dodecahedron for their primitive forms; and all the crystals with two axes, crystallize in the various other forms given in the Table.

Hence we are furnished with the means of deducing the number of axes in crystals from their primitive form, and of approximating to the primitive form when the number of axes is given, by excluding certain other primitive forms which belong to a different number of axes. If HAÜY and BOURNON have given these forms correctly for the following substances, in which I have not detected the number of the axes, they will be arranged as in the annexed Table.

One Axis.	Two Axes.	
Tungstate of lime.	Enclase.	15 Arseniate of copper in obtuse octohedrons.
Octohedrite.	Calamine.	Molybdate of lead.
Ruby silver.	Corundum.	Realgar
Cinnabar.	Staurotide.	
5 Carbonate of strontian.	5 Datolite.	
Dioptase.	Sphene.	
Harmotome.	Titanite.	
Chabasie.	Wernerite.	
Cryolite.	Meionite.	
10 Phosphate of lead	10 Paranthine.	
Glacial sulphuric acid.	Acetate of barytes.	
Carbonate of baryies.	Calomel.	
Pinite.	Arseniate of potash.	
Spinellane.	Green carbonate of copper.	

1. *On the form of the rings or isochromatic curves, and on the nature of the tints in crystals with more than one axis.*

We have already seen that when a crystal has only one apparent axis of double refraction, the isochromatic curves, or lines of equal tint are perfect circles having the axis of extraordinary refraction passing through their centres; but when these curves are the result of two separate axes they assume the more complicated form represented in Pl. XV. fig. 4.

If we transmit polarised light in every possible direction through a crystal, COD *o*, Pl. XV. fig. 5, (which is a section of the sphere in Fig. 4, through the great circle COD*o*,) we shall find that there are two diameters P *p*, P′ *p*′ in which there is neither polarisation nor double refraction. To these lines I have given the name of *resultant axes or diameters of no polarisation*, P,P′, *p*, *p*′ Pl. XV. figs. 4 and 5, being the poles of no polarisation. Each of these poles is surrounded with similar sets of rings, the tints of which commence at P, P′ *p*, *p*′ increase towards O, C, D, A, and B, and reach their maximum at A and B. When the distance PP′ or the inclination of the diameters of no polarisation is considerable, the rings near P,P are almost circular, but the circularity soon ceases as they recede from the pole. If PP′ is less than 90, the tints at C,D, which are always equal, are higher than those at O, and when P,P is exactly 90°, the tints at C,O, *o*, D are equal, and the rings are symmetrical round P,P′.

As PP′ is always less than 90°, the great circle ACBD may be called the *Equator of maximum double refraction or polarisation*, since the double refraction and the polarisation are always greatest in this line. The great circle AOBO,

Pl. XV. figs. 4 and 5, may be called the *meridian of direct polarisation*, because the tints ascend directly from O, o to A and B, and the great circle COD *o* the *meridian of inverse polarisation*, not only because the tints descend from O to P and P′, and ascend from P to C and from P to D, but because there is a real inversion in the character of the tints on each side of PP′.*

In *mica*, *topaz*, and other crystals, where the distance PP′ is 45° and upwards, the system of rings represented in Pl. XV. fig. 4, cannot easily be seen at once; but in *nitre* and other crystals where PP′ is very small, the whole system is finely developed, and every individual curve may be examined with attention.

In order to observe these rings to advantage, let a plate of nitre ACBD, Pl. XV. fig. 6, about $\frac{1}{12}$ or $\frac{1}{15}$ of an inch thick, be cut perpendicular to the axis of the hexaedral prism. If this plate is exposed to polarised light, so that either A B or CD is in the plane of primitive polarisation; and if the transmitted light is analysed with an achromatic prism of calcareous spar, the extraordinary image will exhibit the system of rings shown in Pl. XVI. fig. 7, while the ordinary one will exhibit a system exactly complementary.

The poles P, P′ of no polarisation, are distant 5° 20′, whatever be the thickness of the plate; and through them passes one of the branches CD of the rectangular cross A, B, C, D. The breadth of each ring is least between C and P and D

* The preceding names given to the three great circles, have been drawn from the most prominent physical characters which belong to them. Other names, such as the *meridian of the principal axis*, *the meridian of the secondary axis*, &c. would have been preferable, had they not involved an hypothesis; for it will afterwards be seen that the phenomena may be equally well explained by axes varying both in number and position.

and P, and gradually increases towards AB, owing partly to the different thicknesses at which the light is transmitted through the plate.

When the plate of nitre is turned round its axis, the black cross ABCD immediately breaks up, according to laws which will afterwards be explained; and when AB forms an angle of 45° with the plane of primitive polarisation, the system of rings has the appearance shown in Pl. XVI. fig. 8. The rings themselves have suffered no change by this change of position in the plate; but the black cross is separated into two hyperbolical branches M'P'N', MPN. When the thickness of the plate is above $\frac{1}{10}$ths of an inch, the portions of the rings included between the two hyperbolic branches, are confounded into one mass of white light, while those between C and P, or D and P, are distinctly visible. The concave sides of the hyperbolic branches are strongly fringed with a red and yellow colour, while the convex or inner sides are equally affected with blue rays. By diminishing the thickness of the plate, the rays gradually appear between the hyperbolic branches, but they consist only of pink and green tints, like those in the 5th order of Newton's scale; and even when the rings almost cease to appear by a great diminution of thicknesses, the tints, though at the very commencement of the first order, are not the same within as without the poles of no polarisation. In the position of Pl. XVI. fig. 7, however, and when the tint at O, in the position of Pl. XVI. fig. 8, is only blue of the first order, this irregularity of the tints is almost imperceptible, and the system of rings is distinguished from that produced by crystals with one axis only, by a slight degree of ellipticity. This approximation to the

system shown in Pl. XV. fig. 1, arises from the great attenuation of the crystallized plate, which is no longer capable of developing the tints between the poles of no polarisation; and it is only from the elliptical form of the curves that we can in this state recognise the existence of two axes. Even this ellipticity will disappear by a farther attenuation of the plate; and the crystal deprived as it were of one of its axes, appears to act upon light, exactly like beryl or calcareous spar.

The phenomena which have now been explained, I have found, under various modifications which will afterwards be described, in all the crystals that have more than one axis. The irregularities in the tints are, in every case, developed by increasing the thickness of the plate, and affect the rings which are formed at a considerable distance from the poles of no polarisation. They are analogous to the tints seen along the axis of rock-crystal; and as the secondary forces by which they are produced, conceal the regular action of the principal forces, we must abstract these secondary effects in determining the law according to which the legitimate tints are developed.

2d. On the character, the number, and the position of the axes, by which the tints are produced.

The most important physical circumstance which distinguishes the system of rings formed by different crystals, is the magnitude of the arch PP′, or the inclination of the resultant axes, or diameters of no polarisation; for it is from this angle alone that we can deduce the relative intensity of the real axes, if we suppose them rectangular; or their mutual inclination, if we suppose them equal and inclined.

It would occupy too much room to detail the various practical methods to which I have been obliged to resort in determining the position and inclination of the diameters of no polarisation. In some crystals this process is sufficiently simple, as in topaz, mica, nitre, talc, mother of pearl, prussiate of potash &c. but in other crystals it is extremely difficult, particularly when the angle of inclination is great, and when the specimen is small and its crystalline form indistinct.

The following table contains this angle for 49 crystals. The measures were taken with great care, but some of them are only estimated, and others will admit of correction by the use of better specimens than those I was able to procure.

TABLE *of the inclination of the resultant axes of forty nine crystals.*

	°	′
Sulphate of nickel, certain specimens	3	0
Carbonate of lead	5	15
Nitrate of potash	5	20
Talc	7	24
Mother of pearl	11	28
Hydrate of barytes	13	18
Mica, certain specimens, about	14	0
Arragonite	18	18
Prussiate of potash	19	34
Cymophane	27	51
Borax	28	42
Anhydrite	28	7
Sulphate of magnesia	37	24
——— barytes	37	42
Tincal	38	48
Nitrate of zinc, estimated at about	40	0
Stilbite	41	42
Sulphate of nickel	42	4
Carbonate of ammonia	43	24
Mica	45	0
Lepidolite	45	0
Benzoate of ammonia	45	8
Sulphate of zinc	44	28
——— ammonia	49	42
Sugar	50	0
Sulphate of strontites	50	2
Murio-sulph. magn. and iron	51	16
Sulph. ammon. and magnes.	51	22
Phosphate of soda	55	20
Sulphate of lime	60	0
Oxynitrate of silver	62	16
Feldspar	63	0
Topaz	65	0
Sulphate of potash	67	0
Carbonate of soda	70	1
Acetate of lead	70	25
Citric acid	70	29
Tartrate of potash	71	20
Tartaric acid	79	0
Tartrate potash and soda	80	0
Carbonate of potash	80	30
Kyanite	81	48
Hyper-oxymur. potash	82	0
Muriate of copper	84	30
Epidote, about	87	0
Peridot	87	56
Crystallized Chelt.-salts	88	14
Succinic acid, estimated at about	90	0
Sulphate of iron	90	0

In examining the preceding table, we cannot fail to be struck with the uniform distribution of the angles over the quadrant; a circumstance which contributes to render this new property of crystals a most distinct and valuable physical character in mineralogy.

Having thus determined the angles of the resultant axes, we must now proceed to ascertain the position, the number, and the relative intensities of the real axes, by the combination of whose action the various tints are produced; and in doing this we shall take *mica* as an example.

M. BIOT has announced it as a demonstrated physical truth, that mica has two repulsive axes, one in the plane of the laminæ, and the other perpendicular to the laminæ; and that the polarising force of the first is to that of the second as 100 to 677.* As the experiments from which he deduced this system of forces, consist of observations on the tints, merely in the two rectangular directions, AOB, COD, Pl. XV. fig. 4, we cannot admit them as any thing approaching to evidence for the conclusion which he has drawn; for it is possible that the tints developed in the four quadrants, ACO, ADO, CBO, DBO, may be quite incompatible with such an arrangement. I have observed the tints produced by mica in almost every part of the four quadrants; and it follows from the general law of polarisation which I have discovered, that there is no particular system of forces indicated by the phenomena; but

* "Tous ces résultats étant exactement conformes à ce qu'on observe, il est bien vraisemblable que le système des forces polarisantes du mica est ainsi combiné; mais pour *changer cette présomption en certitude*, il faut exprimer par le calcul les effets d'un pareil système, et voir si la marche des teintes, observées sous chaque incidence, s'y conforme exactement." BIOT *Traité de physique*, tom. iv. p. 557.

that various systems may be proposed which are equally consistent with the observed tints. The number and position of the axes of mica, the rectangularity of the axes, and the nature of the forces which emanate from them, as given by M. BIOT, are therefore entirely hypothetical results.

The phenomena of mica may be explained by the following hypothetical arrangements of the axes.

1. *Rectangular axes.*

1st, By *two inequal negative axes*, Oo, AB, Pl. XV. fig. 4, whose relative intensities are as 100 to 677.

2d, By *a negative axis* Oo, *and a positive axis* CD, whose intensities are as 100 to 173.

3d, By *two inequal positive axes*, CD, AB, whose intensities are as 100 to 117.

2. *Oblique axes.*

4th, By *two equal positive axes*, RS, TV, inclined to each other at an angle of 85° 26′ and

5th, By *two equal negative axes* X, Z, inclined to each other at an angle of 42° 2′.

Hitherto we have supposed that the axes from which the forces emanate, are only two in number; but it will appear from a subsequent part of this paper, that any one axis may be resolved into any even or odd number of axes, by which the phenomena may be explained. All that we know, in short, is that a certain polarising force is exerted at a particular point, but we have no means of ascertaining either the number or the direction of the forces of which it is the resultant. Certain physical circumstances, however, which

will afterwards be noticed, (relative principally to the tints, and the coincidence between some of the hypothetical axes and the principal lines in the crystal,) may lead us to prefer these axes to others; but however plausible the grounds of our preference may be, we must take care not to admit these hypothetical deductions among the number of demonstrated physical truths.

We have already stated that M. BIOT has divided crystals into two classes, attractive and repulsive; and that this division is quite hypothetical, even when applied to crystals with one axis. To the case of crystals with two axes, the classification is wholly inapplicable, for it has been shown, that there are no means of ascertaining either the nature, the number, or the position of the axes. Still, however, the terms *positive* and *negative* may, in most cases, be conveniently employed to mark the principal resulting force which the crystal exhibits. Thus, if we suppose O, Pl. xv. fig. 4, which is the middle point between the two nearest poles of no polarisation to be the position of the principal axis, then mica will be negative, because the tints from O to A, from O to B, from P to C, and from P′ to D, have all a negative character; and topaz will be positive, because the same tints have a positive character. In the case of crystals, however, where PP is 90°, even this limited application of the terms negative and positive entirely fails, and these crystals cannot be considered as belonging to one class more than to another.

3d. On the general law of the tints for all crystals with one or more axes.

When a crystal has one axis of double refraction, the tints are disposed on the surface of the sphere, in regular concentric circles around the real or resultant axes of the crystal; and the intensity of the tint at any point of the spherical surface, is equal to Sin.$^2\varphi$, φ being the distance of the point from the pole of the axis, and the maximum tint in the equator of double refraction being considered as unity. This value of the tint was deduced by M. BIOT, from experiments on sulphate of lime, rock crystal, Iceland spar, and some specimens of mica, before he was acquainted with the discovery of the system of concentric rings; but it was obviously not entitled to any confidence as a general principle, not only from its having been deduced from such a small number of crystals, but from its not representing the phenomena in the very crystals from which it was deduced. In sulphate of lime, for example, Sin.$^2\varphi$ is every where erroneous as the value of the tints. This error indeed is very small in particular azimuths when the distance of the tint from the pole, of what M. BIOT calls the axis, exceeds 30°, but in other azimuths, such as that of 90°, the error is enormous, and in the great circle passing through the plane of the laminæ, the phenomena have no connection whatever, with this law. M. BIOT himself perceived the utter incompetency of the expression Sin.$^2\varphi$ to represent the phenomena in the vertical azimuths, and has expressed the aberrations which he observed in these directions by complicated empirical formulæ. These formulæ, however, though they represent M. BIOT's,

do not at all represent the actual phenomena, for M. BIOT never made a single observation on the tints in the direction of the laminæ, the only direction in which they could be investigated below 30°, from the supposed pole. Had the experiments been made in this way, he would have found that all the observations, instead of being owing, as he supposes, to secondary forces arising from the inequal superposition of the laminæ, are the legitimate results of two axes of double refraction. The experiments of this philosopher upon sulphate of lime must therefore be set aside as incompetent to determine the accuracy of $\mathrm{Sin.}^2\varphi$, as the expression of the tints in crystals with one axis.

As rock crystal possesses secondary forces which interfere with the action of the principal axis, and as there is reason for believing that its apparent axis is only the resultant of two equal negative axes, the value $\mathrm{Sin.}^2\varphi$ cannot be deduced from the valuation of the tints which it developes.

The experiments made by M. BIOT upon calcareous spar, previous to his knowledge of the coloured rings, were better fitted to afford an expression of the variation of the tints; but when we consider that his mode of observation was such that, when applied to arragonite, it gave $\mathrm{Sin.}^2\varphi$ as the law of the tints, and that this mineral has two axes, and cannot therefore have its tints regulated by such a law, we are forced to conclude, that this mode of observation is insufficient even when applied to calcareous spar.

Some new method, therefore, of studying the phenomena of double refraction and polarisation was wanting, in order to determine with certainty, whether any crystal had one or more axes; and what is the law according to which the tints vary in crystals with one axis. By the old mode of observation,

MALUS concluded that *arragonite* and *sulphate of barytes* were crystals with one axis, whereas they have both two axes, and, by M. BIOT's mode of observation, he concluded that *sulphate of lime*, *arragonite*, *topaz*, *sulphate of barytes*, *sulphate of strontian*, and *feldspar*, had only one axis, whereas they have all two distinct axes of double refraction.

The method of observation which I have always employed, consists in observing the system of coloured rings which are seen along the axes of crystals with one axis, and along the resultant axes of crystals with two axes. These rings I first discovered in mica and topaz, about the end of the year 1812, and early in 1813 I discovered in beryl and several other minerals with one axis, the system of rings peculiar to crystals of that class. Dr. WOLLASTON discovered the same system of rings in calcareous spar in 1814; and long after this discovery was made, M. BIOT examined the phenomenon, and showed by measuring the diameters of the rings that in this crystal the variation of the tints was expressed by Sin.$^2\varphi$. By similar measurements of the rings in *zircon*, *ice*, *beryl*, *emerald*, *sapphire*, *ruby*, *rubellite*, *tourmaline*, *apatite*, *vesuvian*, *mellite*, *nepheline*, *muriate of lime*, *muriate of strontian*, *arseniate of copper*, I have found that the expression Sin.$^2\varphi$ applies within the limits of the error of experiments to all these crystals, which, excepting *hydrate of magnesia* and *quartz*, are the only crystals known to have but one axis of double refraction. I therefore consider myself as entitled to set out with this formula, as an expression of the tints for all crystals with one axis, whether their action is of a positive or a negative character.

In proceeding to explain the general law which I have discovered for determining the tints in crystals with any

number of axes, let us suppose that ABC, Pl. xvi. fig. 9, represents the quadrant of a spherical surface, such as we have described in p. 266; and that the position of G, one of the resultant axes, where the tint is nothing, has been carefully determined by experiment, it is required to find the tint at any point E by the action of certain polarising forces which are in *equilibrio* at the point G. If we now suppose,

1st. That the tints are produced by forces emanating from *two negative axes* whose poles are C,A, it is obvious that their relative intensities must be in the ratio of $1 : \frac{1}{\text{Sin.}^2 \text{GC}}$, GC representing half the inclination of the diameters of no polarisation. For as the tint at G produced by A is equal to the tint produced at the same point by C, and since the tint produced there by the axis A is its maximum tint, AG being 90°, then the maximum tint produced by C will be found by the analogy $\text{Sin.}^2 \text{GC} : \text{Rad.}^2 = 1 : \frac{1}{\text{Sin.}^2 \text{GC}}$.

2d. If we suppose that the forces emanate from *two positive axes* A,B, A being greater than B, then the relative intensities must be as $1 : \frac{1}{\text{Cos.}^2 \text{GC}}$.

3d. If the forces emanate from two axes B,C, one of which is positive and the other negative, the intensity of B must be to that of C as $\text{Sin.}^2 \text{GC} : \text{Cos.}^2 \text{GC}$.

Through E draw three great circles AEF, BE and CE, Pl. xvi. fig. 9. and let

T = tint required at the point E.

θ = the arch between the point E and the axis C.

φ = the arch between the points E and B.

a = the tint produced separately at E by the greater axis.

b = the tint produced separately at E by the lesser axis.

ψ = the angle of the forces.

π = the angle B,EF.

ω = the angle CEF.

A = the arch FC, or the angle CAF, or the *azimuth* on the great circle BGC passing through the poles of no polarisation.

D = the arch FE, or the declination or distance of the point E from the same great circle.

ζ = half the difference of the angles at the base or at the diagonal of the parallelogram of forces. Then

1. When the two axes are B,C in the plane passing through the diameters of no polarisation, we have

$$\text{Cos. } \theta = \text{Cos. A} \times \text{Cos. D.}$$

$$\text{Cos. } \varphi = \text{Sin. A} \times \text{Cos. D.}$$

2. When the two axes are C,A in a plane perpendicular to the plane passing through the diameters of no polarisation,

$$\text{Cos. } \theta = \text{Cos. A} \times \text{Cos. D.}$$

$$\varphi = 90^\circ - \text{D.}$$

Then we have, in general, whether the axes are A,C or B,C

$$\text{Cos. } \omega = \frac{\text{Tang. D}}{\text{Tang. } \theta}.$$

$$\text{Cos. } \pi = \frac{\text{Tang. D}}{\text{Tang. } \varphi}.$$

When B,C are the two axes, either both positive or both negative, $\psi = 2\overline{\pi + \omega}$.

When A,C are the axes either both positive or both negative, $\psi = 2(180^\circ - \omega) = 2\omega$.

When B,A are the axes either both positive or both negative, $\psi = 2(180 - \pi) = 2\pi$

When B,C are the axes, the one positive and the other negative,

$$\psi = 180^\circ - 2\overline{\pi + \omega} = 2\overline{\pi + \omega}$$

When A and C are both positive, or both negative, and C > A

$$a = \text{Sin.}^2 \text{CE}, \text{ and } b = \text{Sin.}^2 \text{EA} \times \text{Sin.}^2 \text{GC}$$

When A and B are both positive, or both negative, and A > B

$$a = \text{Sin.}^2 \text{AE}, \text{ and } b = \text{Sin.}^2 \text{BE} \times \text{Cos.}^2 \text{GC}$$

When B and C are the one positive, and the other negative,

$$a = \text{Sin.}^2 \text{EC} \times \text{Cos.}^2 \text{GC}, \text{ and}$$
$$b = \text{Sin.}^2 \text{BE} \times \text{Sin.}^2 \text{GC}.$$

The tints produced separately by each axis being thus determined, the tint resulting from their joint action will be found to be the diagonal of a parallelogram, whose sides are a, b and whose angle is ψ. In order to find this diagonal,

We have $\text{Tang. } \zeta = \frac{a - b \text{ Tang. } \frac{1}{2}\psi}{a + b}$, and

$\zeta + \frac{1}{2}\psi =$ Greater angle at the base;

Hence $T = \frac{a \text{ Sin. } \psi}{\text{Sin. } (\zeta + \frac{1}{2}\psi)}$.

When $a = b$ then $T = 2a \ (\text{Cos. } \overline{\pi + \omega})$

When $a = b$, and the axes equal, then $\pi = \omega$ and

$T = 2a \ (\text{Cos. } 2\pi)$ or $T = 2a \ (\text{Cos. } 2\omega)$ and since $\phi = \theta$

$$T = 2 \text{ Sin.}^2 \phi \ (\text{Cos. } 2\pi)$$

When $\psi = 90^\circ$ then $T = \sqrt{a^2 + b^2}$

When $\psi = 180^\circ$ $\quad T = a - b$

When $\psi = 360^\circ$ $\quad T = a + b$

This general law of the tints may be expressed in the following manner: *the tint produced at any point of the sphere by the joint action of two axes, is equal to the diagonal of a parallelogram, whose sides represent the tints produced by each*

axis separately, and whose angle is double of the angle formed by the two planes passing through that point of the sphere and the respective axes.

If the crystal has three or more axes, the resulting tint produced from any two of them may, in like manner, be combined with the third, and this resulting tint with the fourth, till the general resultant of all the forces is obtained.

If the number of axes with given intensities exceeds *two*, they may be combined by the methods explained in the next section, till they are reduced to two axes, with new relative intensities; and the resultant of all the axes will be obtained by the calculation of the diagonal of a single parallelogram.

The law which we have now explained is obviously deduced from no empirical data, but is rigorously physical, and is founded upon the same principles which regulate the combination of all other mechanical forces. The accuracy with which it represents the complicated system of tints is very wonderful, and cannot fail to recommend it to the reception of philosophers as a true law of nature. In establishing its conformity with the actual phenomena, I shall not content myself with examining it by means of my own experiments. I shall submit it to the severe ordeal of M. BIOT's measurements of the tints of *sulphate of lime*, taken long before the discovery of the law, and which he considered as the result of irregular action depending upon imperfect crystallization. To those who may desire farther evidence, a still more decisive trial may be offered; a trial too, in which the eye itself is capable of recognising the perfect identity between the observed and calculated results. If we compute all the tints,

by means of the law, for any crystal in which the rings round the resultant axes can be seen at one view, and project them upon paper, after they are reduced to different thicknesses, corresponding to the oblique transit of the rays through the parallel plate, we shall have a representation of the rings actually observed, expressing in the most accurate manner all the inversions of the tints, and exhibiting the points of contrary flexure, and the innumerable varieties of form which the curves assume.

In order to determine the tints of sulphate of lime at great obliquities, M. BIOT placed the laminæ in a tube shut up at both ends by plates of glass; and by means of a metallic rod he was able to fix them at any angle with the axis of the tube. He then filled the tube with water, or oil of turpentine, and observed the tints at inclinations as high as seventy-eight degrees, two minutes. In this way he constructed a formula which represented these observations, and by means of this formula he computed the following table, in which I have reduced his numbers, in order to represent the tints at equal thicknesses, by dividing them by the secants of the angles of refraction.

TABLE *of the tints of sulphate of lime in different azimuths reduced from* M. BIOT's *experiments.*

Angle of refraction	Angular dist. from the axis.	Azimuth of 0°	Azimuth of 22° 30′	Azimuth of 45°	Azimuth of 67° 30′	Azimuth of 90°
0°	90°	1.0000	1.0000	1.0000	1.0000	1.0000
10	80	0.9768	0.9787	0.9838	0.9905	0.9944
20	70	0.9105	0.9164	0.9352	0.9615	0.9756
30	60	0.8101	0.8213	0.8600	0.9117	0.9434
40	50	0.6885	0.7047	0.7664	0.8523	0.9002
50	40	0.5559	0.5751	0.6607	0.7794	0.8482
60	30	0.4241	0.4409	0.5483	0.6982	0.7938
70	20	0.3027	0.3043	0.4121	0.5949	0.7337
80	10	0.2077	0.1599	0.2501	0.4660	0.6184
90	0					

In determining the tints by new experiments, I adopted two different methods of investigation. By one of these, which was analogous to that of M. BIOT, I observed the tints for all angles of refraction up to 60°, and by another mode I observed the remaining tints up to 90°, and I endeavoured to avoid as much as possible the secondary effects produced by obliquity.

The first of these methods is shown in Pl. xvi. fig. 11, where ABCD is a plate of sulphate of lime, whose natural surfaces, AB, CD, are parallel to the plane passing through the resultant axes. Upon these surfaces I cemented two prisms of crown glass M,N, by a thin and equal film of Canada balsam, and having placed them upon the goniometer, I was able to observe the tints with great correctness at angles of refraction considerably beyond those which could be obtained in air. At angles of refraction between 60° and 90°, I resorted to the second method, which consisted in transmitting the polarised light through the parallel faces AD, AC. This method is attended with peculiar difficulties; and much perplexing labour must be submitted to before good plates can be obtained, as the laminæ are constantly separating from each other; and by admitting the materials employed in grinding and smoothing the surfaces, the transparency of the plate is destroyed. I found it necessary, indeed, to bind the laminæ together by wax, sometimes by plates of wood, and at other times by placing them in a small hand vice. By these means my experiments at last succeeded, and all the mysterious actions of sulphate of lime, with the origin and classification of which M. BIOT had been so much perplexed, were immediately unravelled. The two resultant axes and their attendant rings were observed at an angular distance of 60°; the relative intensities of the two axes, from which all the irregularities arose, were thus given,

and the actual progress of the tints became a matter of simple observation. In this way I constructed the following table.

Angle of refraction	Angular dist. from the axis.	Azimuth of 0°	Azimuth of 22° 30′	Azimuth of the resultant axis 30°	Azimuth of 45°	Azimuth of 67° 30′	Azimuth of 90°
0°	90°	1.0000	1.0000	1.000	1.0000	1.0000	1.0000
10	80	0.978	0.979	0.980	0.984	0.992	0.995
20	70	0.913	0.919	0.927	0.938	0.962	0.973
30	60	0.811	0.824	0.835	0.861	0.913	0.939
40	50	0.689	0.710	0.729	0.768	0.853	0.898
50	40	0.558	0.576	0.588	0.662	0.782	0.850
60	30	0.435	0.438	0.448	0.538	0.719	0.810
70	20	0.340	0.294	0.304	0.410	0.659	0.775
80	10	0.274	0.125	0.154	0.305	0.614	0.760
90	0	0.250	0.105	0.000	0.240	0.597	0.753

In the preceding table I have not given the value of the tints to more than three decimal places, as even the third decimal place is partly the result of estimation.

To those who may repeat these experiments, it will be necessary to state, that in almost all crystals with two axes, the tints in the neighbourhood of the resultant axes, when the plate has a considerable thickness, lose their resemblance to those of NEWTON's scale, as will be more minutely described in another paper. The rings, however, are perfectly formed; and the numbers in the table are the values of the tints deduced from their position, and not from their actual colour. Thus, in the third ring or order of colours, reckoned from the resultant axes, I call the value of the middle point about 17, although the tint is not a yellowish green, as in NEWTON's scale. This mode of proceeding is strictly correct, for the cause which prevents the tint from being a yellowish green, disappears in general by diminishing the thickness of the plate.

In comparing these observations with the general law, we shall suppose that one of the axes has the same situation in

the laminæ as the line which M. BIOT calls the axis,* when the other axis is perpendicular to the plane of the laminæ. These positions are indicated in Pl. XV. fig. 4, by O and A, and in fig. 9, by C and A. Since GC, therefore, or half the distance of the resultant axis is 30°, we shall have the relative intensity of the axis C, to that of A, as $\frac{1}{\text{Sin.}^2 GC}$: or as 4 to 1. With these data the following table has been calculated.

Angle of refraction.	Angular distance from the axis.	Azimuth of 0°.	Azimuth of 22° 30′.	Azimuth of the resultant axis, 30°.	Azimuth of 45°.	Azimuth of 67° 30′	Azimuth of 90°.
0°	90°	1,0000	1,0000	1,0000	1,0000	1,0000	1,0000
10	80	0,9775	0,9796	0,9811	0,9848	0,9903	0,9925
20	70	0,9124	0,9202	0,9258	0,9401	0,9614	0,9707
30	60	0,8125	0,8272	0,8385	0,8660	0,9160	0,9375
40	50	0,6901	0,7093	0,7254	0,7729	0,8573	0,8967
50	40	0,5600	0,5755	0,5936	0,0593	0,7901	0,8533
60	30	0,4375	0,4356	0,4507	0,5339	0,7221	0,8125
70	20	0,3378	0,2980	0,3018	0,4071	0,6615	0,7792
80	10	0,2726	0,1238	0,1511	0,2983	0,6190	0,7575
90	0	0,2500	0,1035	0,0000	0,2500	0,6036	0,7500

The agreement between the numbers in this and the preceding table, will convey an idea of the accuracy with which the law accommodates itself to all the capricious changes of the tints. I have compared it also with numerous experiments made on *nitre*, *mica*, *topax*, *sulphate of iron*, *&c.* embracing all the varieties in the inclination of the resultant axes, and have found it equally accurate in all these crystals. The projection of the calculated results, indeed, compared with the system of rings where their curvature is the most variable, may be considered as an ocular demonstration of the correctness of the law.

* It is a curious fact, that sulphate of lime is the only crystal, out of a great number, in which the principal axis does not coincide with any of the prominent lines in its primitive form, as ascertained by HAUY. Does not this prove incontestibly that its primitive form is at present undetermined?

SECT. IV. *On the resolution and combination of polarising forces, and the reduction of all crystals to crystals with two or more axes.*

It has been remarked by M. BIOT in one of his latest memoirs, that it has been established by LAPLACE, that if the Huygenian law is admitted, the extraordinary refraction is *necessarily* produced by a repulsive force; and the same philosopher has also asserted, that from the opposite character of the tints produced by beryl and quartz, there results *necessarily* this alternative: "Either the forces which produce the extraordinary refraction are repulsive in one of the classes and attractive in the other, or they are repulsive in both; but they turn the axes of the luminous molecules in directions inversely rectangular. New experiments have proved to me that the first mode is that which nature realises." *

This view of the subject of double refraction and polarisation has always appeared to me erroneous. I have never been able to perceive that the phenomena of calcareous spar were *necessarily* referable to a repulsive force, or that nature had restricted herself to any of the alternatives which have just been stated. These opinions have acquired new strength as I advanced in the inquiry, and I trust I shall be able to demonstrate, not only that the phenomena of double refraction and polarisation may be explained by forces or combinations of forces different from those which have been given by LAPLACE and BIOT, but that there are certain analogies of nature, and certain physical circumstances in the phenomena, which

* *Mem. de l'Institut.* Lu, 2 Jan. 1815.

may lead us to select one combination of forces in preference to others, as the means which nature has employed in the accomplishment of her purposes.

If we consider a material particle in motion as under the influence of forces, the nature and the source of which are unknown, we may ascribe any change of direction which it experiences, either to a single attractive, or a single repulsive force, emanating from different sources; or we may regard it as the resultant of a variety of forces of the same, or of opposite characters. In the phenomena of the solar system, a repulsive force is necessarily excluded by the simplest considerations. In the reflection of light, the return of the ray cannot, without the most manifest absurdity, be ascribed to an attractive force residing without the reflecting surface; and for the same reason, the refraction of the transmitted light cannot be considered as the effect of a repulsive force existing without the transparent medium. But the case is quite different in the phenomena of double refraction and polarisation. There are here no prominent physical circumstances which can lead us to a general determination of the nature of the forces. The deviation of the extraordinary ray in beryl, may be the result of a repulsive force emanating from the axis of the prism, or of an attractive force emanating from two equal rectangular axes lying in a plane perpendicular to the axis of the prism, or of various other combinations of forces, either of the same or of opposite names.

We shall now proceed to demonstrate the first of these positions, namely, that the action of two equal rectangular axes of a positive character, as calculated by the law of polarisation already explained, is the same as the action of

one negative axis of the same intensity as either of the other two, and placed at right angles to the plane of the positive axes.

Let A,B,C, fig. 9, be the poles of the three axes of which B,C are the positive axis, and A the negative axis.

The tint produced at any point E by the axis A alone, is of the same intensity, and the same character, as the tint that would be produced at the same point by the positive axes B,C acting jointly. Through E draw the great circles AEF, BE, and CE, and call EF$=x$, BE$=\phi$, EC$=\theta$, BF$=m$, CF$=n$, BEF$=\pi$, CEF$=\omega$. Then as the tint produced at E by the axes A alone is Sin.^{2} AE or Cos.$^2 x$, we must show that the resulting tint produced by the two axes B,C, is also equal to Cos.$^2 x$. By spherical trigonometry we have

$$\text{Sin.}^2\phi \times \text{Sin.}^2\pi = \text{Sin.}^2 m$$

$$\text{Sin.}^2\theta \times \text{Sin.}^2\omega = \text{Sin.}^2 n$$

But since $m + n = 90°$ Sin.$^2 n =$ Cos.$^2 m$. Hence

$$\text{Sin.}^2 m + \text{Sin.}^2 n = 1$$

and by adding together the two first equations we have

$$\text{Sin.}^2\phi \times \text{Sin.}^2\pi + \text{Sin.}^2\theta \times \text{Sin.}^2\omega = 1.$$

Again, since $\text{Tang.}\,\phi = \frac{\text{Sin.}\,\phi}{\text{Cos.}\,\phi}$, and Tang. $x =$ Tang. $\phi \times$ Cos. π and Cos. $\phi =$ Cos. $x \times$ Cos. m, we obtain by substitution

$$\text{Tang.}\,x = \frac{\text{Sin.}\,\phi \times \text{Cos.}\,\pi}{\text{Cos.}\,x \times \text{Cos.}\,m} \text{ and}$$

Tang. $x \times$ Cos. $x \times$ Cos. $m =$ Sin. $\phi \times$ Cos. π; but since

Tang. $x \times$ Cos. $x =$ Sin. x we have

Sin. $x \times$ Cos. $m =$ Sin. $\phi \times$ Cos. π, and by the same reasoning

Sin. $x \times$ Cos. $n =$ Sin. $\theta \times$ Cos. ω.

Hence, after squaring both equations and adding them together, we have

$$\overline{\text{Sin.}^2 x \times \text{Cos.}^2 m} + \overline{\text{Sin.}^2 x \times \text{Cos.}^2 n} = \overline{\text{Sin.}^2 \varphi \times \text{Cos.}^2 \pi} + \overline{\text{Sin.}^2 \theta \times \text{Cos.}^2 \omega}$$

$$\text{and Sin.}^2 x = \frac{\overline{\text{Sin.}^2 \varphi \times \text{Cos.}^2 \pi} + \overline{\text{Sin.}^2 \theta \times \text{Cos.}^2 \omega}}{\text{Cos.}^2 m \times \text{Cos.}^2 n}$$

But $\text{Cos.}^2 m \times \text{Cos.}^2 n = 1$. Hence

$$\text{Sin.}^2 x = \overline{\text{Sin.}^2 \varphi \times \text{Cos.}^2 \pi} + \overline{\text{Sin.}^2 \theta \times \text{Cos.}^2 \omega}.$$

But since $\text{Cos.}^2 \pi = \text{Cos.} 2\pi + \text{Sin.}^2 \pi$ and

$\text{Cos.}^2 \omega = \text{Cos.} 2\omega + \text{Sin.}^2 \omega$ we have

$$\text{Sin.}^2 x = \text{Sin.}^2 \varphi \times \text{Cos.} 2\pi + \text{Sin.}^2 \varphi \times \text{Sin.}^2 \pi + \text{Sin.}^2 \theta \times \text{Cos.} 2\omega + \text{Sin.}^2 \theta \times \text{Sin.}^2 \omega$$

But $\text{Sin.}^2 \varphi \times \text{Sin.}^2 \pi + \text{Sin.}^2 \theta \times \text{Sin.}^2 \omega = 1$; hence

$\text{Sin.}^2 x = \text{Sin.}^2 \varphi \times \text{Cos.} 2\pi + \text{Sin.}^2 \theta \times \text{Cos.} 2\omega + 1$, and

Since $\text{Cos.}^2 x = 1 - \text{Sin.}^2 x$ we have

$$-\text{Cos.}^2 x = \text{Sin.}^2 \varphi \times \text{Cos.} 2\pi + \text{Sin.}^2 \theta \times \text{Cos.} 2\omega$$

Now in the parallelogram of forces, the two forces AC, AD (Pl. xvi. fig. 12) are $\text{Sin.}^2 \varphi$, $\text{Sin.}^2 \theta$ when the axes are of equal intensity, that is $AC = \text{Sin.}^2 \varphi$, and $AD = \text{Sin.}^2 \theta$. But

$\text{Sin.}^2 \varphi : \text{Sin.}^2 \theta = \text{Sin.}^2 \omega : \text{Sin.}^2 \pi$, and the

Angle $CAD = 2\pi + 2\omega$. Hence

$CAB = 2\pi$ and

$BAD = 2\omega$

Consequently $AF = \text{Sin.}^2 \varphi \times \text{Cos.} 2\pi$ and

$BF = \text{Sin.}^2 \theta \times \text{Cos.} 2\omega$, and therefore

$AF + BF = AB = \text{Sin.}^2 \varphi \times \text{Cos.} 2\pi + \text{Sin.}^2 \theta \times \text{Cos.} 2\omega$

which is the very same value which we have found above for $\text{Cos.}^2 x$, the measure of the tint produced by one axis.

Hence it follows that the intensity of the tints, and consequently the form of the curves of equal tint, are the same, whether they are produced by one negative, or by two equal and rectangular positive axes.

We may therefore conclude,

1st. That a single negative axis may be resolved into two

equal rectangular positive axes of the same intensity as the negative axis, and lying in a plane perpendicular to that axis.

This fundamental principle being established, we may proceed still farther in the resolution of the axes into axes of equal intensity, but differing in their character and position. Thus in Pl. xvi. fig. 9, the negative axis A being equivalent to the positive axes B,C, we may again resolve B into a negative axis at C and another at A. Hence the original axis at A is equivalent to a negative axis at A, a positive axis at C, and a negative axis at C. But these two last axes destroy each other; and therefore the negative axis A is the only one that is left. If we also resolve the other positive axes C into two negative axes at A and B, we have the original negative axis A, equivalent to four negative axes, two at A, one at C, and one at B. We therefore conclude,

2d. That the effect of a single negative axis may be represented by three rectangular negative axes, provided two of them are equal, and the third has a greater intensity than the other two; and *vice versa* a single positive axis may be represented by three rectangular positive axes, provided two of them are equal, and the third has a less intensity than the other two.

Hence it follows that by leaving one axis at A, the effect of the other three negative axes must be to destroy each other, a result which will afterwards be established in a different way, and will lead us to some important consequences. The same is true of positive axes, *mutatis mutandis*.

Hitherto we have resolved the axes of crystals into other axes differing in position and character, but having the same intensity. If we consider, however, that any one axis at A

is equal to any number of equal or inequal axes of the same name placed at A, the sum of whose intensities is equal to the intensity of the axis at A, it will follow—

3d. That the effect of a single axis may be represented by any even number of equal axes of an opposite name, provided in every pair of axes one of them is at right angles to the other, and all of them lie in the same plane perpendicular to the original axis. The intensity of each of the new axes must be to that of the single axis as $\frac{2}{n}$ to 1, n being the number of axes.

4th. That the effect of a single axis may be represented by any number of equal pairs of axes of an opposite nature, provided the axes which compose each pair are equal and rectangular, and all of them lie in the same plane perpendicular to the original axis. The intensity of the axes which compose each pair may differ in any way, but $\frac{s}{2}$ or half the sum s of the intensities of all the axes must be equal to the intensity of the single axis.

In the preceding observations we have supposed the axes of the crystals to be rectangular, a supposition which is by no means rendered necessary by the phenomena. It is obvious indeed that the effect of a single axis cannot be represented by two axes that have any other inclination but that of 0° or 90°; but we shall presently see that the effect of two rectangular axes may be represented by two equal and inclined axes. Let ABC, Pl. xvi. fig. 10, be a spherical triangle representing the eighth part of the sphere, and having all its angles right angles. Then if G is the pole of one of the resultant axes or diameters of no polarisation, the angular distance $\alpha\beta$ of the two inclined axes α, β must be such, that the angle $\alpha G\beta$ is a

right angle, for it is only at this angle that the two axes can destroy each others' actions, and produce a resultant axis at G. Consequently, since αGF is 45°, we have Tang. $\frac{1}{2}\alpha\beta$ = Sin. GF. = Cos. AG. The same reasoning is applicable if the axes are situated at a, b, $a\,G\,b$ being a right angle; but in this case Tang. $\frac{1}{2}\,a\,b$ = Sin. AG. Hence,

5th. The action of two inequal rectangular axes is equal to the action of two equal axes, the tangent of the half of whose inclination is equal to the cosine of half the angle of the resultant axes, if the two equal axes are situated in the plane BC, or to the sine of half that angle if they are situated in *ab*.

In order to ascertain the intensity of the axes α, β, or a, b, we must assume a position and a character for the rectangular axes which they represent. Let the two rectangular axes, therefore, be a negative axis at A, and a positive axis at F, whose relative intensities will be Cos.^{2} GA : Sin.^{2} GA, and let A be the most powerful axis. Then the intensity of either of the axes α, β will be to that of A as $\frac{1}{2\,\text{Sin.}^2\frac{1}{2}\,\alpha\beta}$: 1; and the intensity of either of the axes a, b will be to that of A as $\frac{1}{2\,\text{Cos.}^2\frac{1}{2}\,a\,b}$: 1. Hence when $\alpha\beta = 90°$. we have $\frac{1}{2\,\text{Sin.}^2\frac{1}{2}\,\alpha\beta} = 1$, which brings us back to Case 1st. When $ab = 0°$. we have $\frac{1}{2\,\text{Cos.}^2\frac{1}{2}\,a\,b} = \frac{1}{2}$, for as the two axes a, b coincide with A, their sum must be equal to A, or $a + b = 1$, and since $a = b$, we have $a = \frac{1}{2}$. When B is the most powerful axis, the preceding ratios of the intensities must be interchanged.

If the two inclined axes α, β are supposed to be inequal, they may have an infinite number of positions in the great circle passing through α, β; but their relative position must be such, that the great circles passing through each of them,

and the resultant axis must intersect each other at an angle of 90°, and their intensities will be as the squares of the sines of their respective distances from the resultant axis. The ratio of the intensities, however, can never exceed that of 1 to Sin.$^2\phi$, ϕ being the distance of the resultant axis from the plane passing through α, β.

It would be needless to pursue this subject any farther. I have briefly illustrated the general principles of the resolution of the axes of crystals, and the reader will have no difficulty in deducing other combinations by which the phenomena may be represented. A very important question is naturally suggested by the results to which we have arrived. Are there any physical circumstances either of a general or a particular nature which may lead us to ascertain the real position of the axes of crystals, and to determine the character of the forces by which the phenomena of polarisation are produced? When we consider the case of *Iceland spar*, we perceive no peculiarities which can induce us to refer its polarising force to two or more positive axes in preference to a negative axis; but in *rock crystal*, the secondary tints discovered by M. BIOT along the axes of the prism, seem to indicate that its apparent positive axis is merely the resultant of two or more equal and rectangular negative axes. M. BIOT indeed ascribes these secondary effects to new forces independent of the principal polarising force; but I have discovered them also in crystals with two axes, and have observed some phenomena which seem to prove that they have their origin in the unbalanced action of the two principal axes.

With regard to the nature of the forces we are not left

entirely without some general indications. In magnetism and electricity, the various phenomena are produced by two opposite and co-existent forces which modify each other's action; and since opposite forces are obviously indicated by the phenomena of polarisation, we have the strongest reasons, from analogy, to believe that they are also co-existent in crystals. The remarkable phenomena exhibited during the transmission of heat along plates of glass, give additional weight to the deductions of analogy. Here the negative structure invariably accompanies the positive structure, and the plates which are under the influence of these forces, exhibit phenomena precisely the same as artificial magnets.* It will, however, be naturally asked, what has become of the negative force in calcareous spar, if it is supposed to have two equal and rectangular positive axes? To this we reply, that the negative force, originally less than the positive forces, has been balanced by the opposite actions of the two positive axes; and that the resulting force, which has all the characters of a negative force, is merely the difference between the original negative force and the negative force which is equivalent to the effect of the positive axes. Nor is this mere speculation, for while it is an arrangement of the polarising forces which is as likely as any other to be the one actually followed by nature, it is almost directly supported by a series of experiments, which I have made on plates and cylinders of glass.†

* *Phil. Trans.* 1816, p. 64, 83, 84, &c.

† See the *Edinburgh Transactions*, vol. VIII. p. 353.

SECT. V. *On the polarising structure of crystals that have the cube, the regular octohedron, and the rhomboidal dodecahedron for their primitive form.*

In examining the structure of doubly refracting crystals, it will be found that there are *thirteen* bodies which seem to be entirely destitute of the polarising structure, and other *nine* which sometimes exhibit distinct traces of two opposite structures.* And it is a very curious circumstance, that the crystals which have this character are those which crystallize in the form of a cube, a regular octohedron, and a rhomboidal dodecahedron, as appears from the following table.

Names of crystals.	*Primitive form.*	*Names of crystals.*	*Primitive form.*
Garnet	Rhomboidal dodecahedron	Boracite†	Cube
Blende	Rhomboidal dodecahedron	Muriate of soda	Cube
Diamond	Regular octohedron	Muriate of potash	Cube
Spinelle ruby	Ditto	Leucite	Cube
Ceylanite	Ditto	Analcime	Cube
Alum	Ditto	Nitrate of barytes	
Muriate of ammonia	Ditto	Sulphate of alumine and ammonia	
Fluor spar	Ditto	Nitrite of lead	
Ruby copper	Ditto	Cinnamon stone	
Nitrate of lead	Ditto	Essonite	
Nitrate of strontian	Octohedral crystals	Uranite	

These crystals, as will be seen by a comparison with HAÜY's table of primitive forms,‡ include all the transparent and translucent crystals which he has arranged under the preceding forms, excepting *tungstate of lime;* but this ex-

* See the *Edinburgh Transactions,* vol. VIII. p. 157, where I have given an account of the discovery of this property.

† Boracite has an axis in every direction, like the waxen partitions of the honeycomb.

‡ *Traité de Mineralogie,* tom. I. p. 273.

ception is removed, as he has since found that this mineral has for its primitive form an irregular octohedron.

In the paper already quoted, I have described the leading optical properties of this class of crystals; but I have since observed several new phenomena which throw additional light on the subject. In the examination of thirty very fine cut diamonds of great size and value, I found several which had no action whatever upon light. Many of them polarised a fine sky blue tint of the first order, with a complementary straw yellow, in whatever position they were held; and one exhibited a succession of five or six tints, which were not the exact tints of NEWTON's scale, but similar to those seen near the resultant axes of crystals. In a diamond with natural faces, which has the form of a fine regular octohedron, there were no indications whatever of the polarising structure. In two specimens of *leucite*, broad segments of the coloured rings were developed when the light was transmitted in different directions; and *blende* and *analcime*, even at thicknesses less than $\frac{1}{25}$ of an inch, displayed a considerable action upon polarised light.

M. BIOT has endeavoured to account for the absence of polarisation and double refraction in this class of crystals by remarking—"that the phenomena of the attractive class are explicable by a prolate ellipsoid and those of the repulsive class, by an oblate ellipsoid; and that the sphere, forming a passage from one of these limits to the other constitutes a sort of neutral state, and corresponds to those crystals which, crystallizing in the regular octohedron or the cube, are destitute of the property of double refraction."*

* *Traité de Physique*, tom. iv. p. 349, Paris, 1816.

This remark, however, is by no means an explanation of the fact. It is merely a different mode of expressing the fact, that the cubical and octohedral crystals have no double refraction; for the sphere is applicable to the optical action of air and all other fluids, whether aeriform or liquid, which have not the property of double refraction.

The discovery of the remains of polarising axes in a great number of crystals of this class, completely proves that, like all other crystallized bodies, they actually possess the doubly refracting and polarising structure, but that this structure entirely vanishes in some specimens, by the equilibrium of the forces in every point of the crystal, and reappears in some specimens when that equilibrium is not complete.

The principles of the resolution of polarising forces which we have already explained, indicate the manner in which this equilibrium is effected. Three equal and rectangular axes, either all of the negative, or all of the positive character, will produce a perfect equilibrium, or mutually destroy each other at every point of the crystal; for since two equal positive axes are equivalent to one negative axis, of the same intensity as either of the two; and since this negative axis will be balanced by an equal positive axis coincident with it, it follows, that the three rectangular positive axes will be in equilibrio. In perfect crystals of this class where all the axes are equal, and their position exactly rectangular, the equilibrium of the forces is complete; but if the axes are not equal, or their position not accurately rectangular, the phenomena of one or more axes will be developed. If one axis, for example, is weaker than the other, then the result will be the appearance

of an axis of an opposite name to that of the three axes and coinciding with the resultant of the other two; but if one of the axes is stronger than the other two, the result will be an axis of the same name with the three axes, and coinciding with the strongest axis.

Let us now consider what will be the effect if the three negative axes C,A,O, Pl. xv. fig. 4, are all different; C being the strongest and O the weakest. The two axes C and A will, by their joint action, produce resultant axes P,P′ lying in the plane COD. Let the third axis O be resolved into two positive axes at C and A, then we have a negative axis A, and a positive axis equal to O acting at A, and the negative axis C, and a positive axis equal to O acting at C; but O is less than A or C, therefore we have a negative axis equal to A — O acting at A, and a negative axis C — O acting at C; and as the last of these is the strongest, the general effect will be the production of two resultant axes in the plane COD. But as C — O has to A — O a greater ratio than C has to A, the poles of no polarisation P,P′ will be removed farther from O than they were by the action of C and A alone.

The equilibrium of the three axes may also be disturbed by another cause, namely, by a deviation from perfect rectangularity. In order to understand the effect of this irregularity, let ABC, Pl. xvi. fig. 10, be the three rectangular axes, and Bα, Cβ the deviation of the axes α and β. Then the effect of the equal and inclined axes α, β is to produce poles of no polarisation, one of which is seen at G in the plane FGA, bisecting the angle formed by the planes passing

through the axes. If the third axis is rectangular to α and β, or placed at A, the effect of it would be to throw the pole G farther from A towards F; but as it is not probable that the deviation of α and β would be in a plane exactly at right angles to A, while A kept its situation; then if A deviates from A, or if while A retains its position, the deviations of α and β are not in the plane BC, an irregular system of curves will be produced, differing essentially from those which are formed by two axes.

Here then we have three leading causes of the destruction of the equilibrium of the three axes. In consequence of the first, tints of a single structure will be developed, as in some specimens of diamond and alum. In consequence of the second, the opposite tints of a double structure will be developed, as in several specimens of diamond, fluor spar, muriate of soda, semiopal, and analcime; and in consequence of the third, a confused system of rings will be exhibited, such as I have discovered in several specimens of leucite.

The coincidence of the preceding deductions, with the experimental results, affords a strong presumption, that we have been tracing the actual operations of nature. In the phenomena of crystals both with one and with two axes, we have seen that these axes are coincident with some permanent line in the primitive or secondary form of the crystal. This is the case in every crystal with one axis that I have examined; but it is not easy to establish this coincidence in crystals with two axes, for it is impossible to fix the situation of the axes by which the phenomena are produced. In almost every case, however, the most probable position of the axes is

coincident with some prominent line, either in the primitive or secondary forms.*

The same coincidence ought therefore to be expected in the case of the cubical, octohedral, and rhomboido-dodecahedral crystals; and it is a singular fact, that the *cube*, the *octohedron*, and the *rhomboidal dodecahedron*, are the only regular geometrical solids in crystallography, in which neither more nor less than three equal rectangular axes can be placed symmetrically. In the cube, for example, each of three axes is perpendicular to the three pair of square surfaces by which the solid is contained. In the *regular octohedron* each of them coincides with the line which joins the six solid angles of the figure; and in the *rhomboidal dodecahedron*, each of the rectangular axes passes through the six solid angles, each of which is contained by four acute angles of the rhomboidal planes.

SECT. VI. *On the artificial imitation of all the classes of doubly refracting crystals by means of plates of glass.*

In the Philosophical Transactions for 1816, I have given a full account of the very remarkable phenomena which are

* Malus seems to have believed that the axis of extraordinary refraction was necessarily coincident with some prominent line in the primitive form. We quote the following passage in confirmation of our general views, though there can be no doubt that the generalisation which it implies is premature. "Dans le rhomboide, l'axe de réfraction se confond avec l'axe du crystal; mais dans les autres formes on n'a pas de données suffisantes pour le déterminer à priori. Cependant le nombre des directions entre lesquelles on peut balancer est toujours très-borné. Dans l'octaèdre à triangle scalène, par exemple, on est assuré d'avance que l'axe de réfraction est un des trois axes rectangulaires de la forme primitive." *Théorie de la Double Réfraction*, p. 177.

exhibited during the propagation of heat along plates of glass and other uncrystallized bodies; and of the method of communicating permanently to these substances, all the properties of regular crystals. Since that paper was written, I have completed a series of new experiments on the distribution of the polarising force in prepared glass, which led me to the determination of the laws by which all the phenomena can be calculated. These laws are precisely the same as those which we have already deduced in the preceding sections; and there is not one phenomenon belonging to regular crystals which I have not been able to imitate in glass.

In these artificial crystals, however, the centre of the sphere is infinitely distant, and therefore the sine of the angle which the refracted ray makes with the axis in the regular crystals, is equivalent in the artificial ones to the distance of the ray from the axis, and is replaced by this quantity in the new formulæ. Thus if T is the tint corresponding to any given distance D from the axis, then the tint t at any other distance, d is $t = \frac{T d^2}{D^2}$ a formula which represents accurately the progress of the tints whether the axis is negative or positive.

If in rectangular plates of glass D represents the distance of any of the black fringes, or the lines of no polarisation MN, OP, Pl. xvi. fig. 13, from the axis *ab* of the plate, and T the maximum central tint; then if we conceive an axis situated perpendicular to the plate in every point of the line *a b*, and an axis in the plane of the plate perpendicular to the former axis, T will be the tint produced in every part of the plate by the axis in its plane when polarised light is transmitted through it perpendicularly. But as this tint, whose value is

T, is counteracted at m in every part of the line MN, by the action of the axes at A, &c. perpendicular to the plate, the intensity of the perpendicular axis at the distance A m = D, will also be equal to T. Hence, in order to find the tint t produced by this perpendicular axis at any other distance A $c = d$, we have $t = \frac{T\,d^2}{D}$ by the law for one axis; but as this tint is counteracted by the tint T produced at c by the axis in the plane of the laminæ, we have the resulting tint or $t = T - \frac{T\,d^2}{D^2}$.* Here the law is very simple, and the tints are all disposed in straight lines, in consequence of the angle of the forces being 180°, and the number of the axes infinite. This case is precisely the same as that of regular crystals when the tints are calculated in the plane COD, Pl. xv. fig. 4, by means of two axes at O and A, the effect of the axis A is the same at every point in the line COD, and therefore the resulting tint, at any point, is the uniform tint produced by A (which is the same as the tint at O) minus the tint produced by the axis O alone. The law of the progression of the tints therefore in rectangular plates of glass, is exactly the same as in crystals with two axes; and we have the same difficulty in determining whether the axes are of the same or of opposite names.

The similarity between the various phenomena of the real

* This formula is the same as that which I had deduced long ago from experiment. M. BIOT had also obtained from observation the very same expression of the tints, and about the same time. The formula which I have employed was more general than that of M. BIOT, as I had found the term D to be a function of the breadth of the plate. My formula was therefore $t = T - \frac{T\,d^2}{.3128\,B^2}$, B being the breadth of the plate; or if T is the maximum tint given by the plate, we have $t = T - \frac{T\,d^2}{.188\,B^2}$. See *Edinburgh Transactions*, Vol. VIII. Art. XVIII.

and the artificial crystals is still more striking than the agreement of the laws of the tints, as all the different classes of crystals can be imitated in glass. This similarity will appear from the following comparison:

Artificial crystals.	*Real crystals.*
1 A circular plate of glass crystallized by the transmission of heat.	1 Crystals of zircon, quartz, ice, &c.
2 A circular plate of glass heated, and in the act of cooling.	2 Crystals of calcareous spar, beryl, ruby, &c.
3 A rectangular plate of glass when heat is in the act of being transmitted through the plate.	3 Crystals of topaz, sulphate of lime, &c. in which the principal force is attractive.
4 A rectangular plate of glass heated, uniformly, and in the act of cooling.	4 Crystals of mica, nitre, &c. in which the principal force is repulsive.
5 A circular plate of glass, permanently crystallized to a slight degree, and then heated, so as to produce, in the act of cooling, a tint equal to its permanent tint.	5 Cubical, octohedral, and rhomboido-dodecahedral crystals, in which the polarising force is destroyed by opposite actions.

Although these artificial crystals exhibit in the most perfect manner all the phenomena of fixed and moveable polarisation, and develope the tints of NEWTON's scale with much more regularity than even sulphate of lime, yet M. BIOT has described the phenomena which they exhibit under the head of *imperfect crystallization*. If such a distinction were neces-

sary for the proper classification of the phenomena, I would have no hesitation in reversing the name, and giving the appellation of imperfect crystallization to the phenomena produced by *sulphate of lime*, and the greater number of crystals of the mineral kingdom. If we employ pieces of pure glass of regular forms, and with straight and smooth edges, and if we transmit through them the heat in a uniform and careful manner, we shall obtain a structure which will develope all the phenomena of polarisation with the utmost beauty and perfection.

There is one point, however, and a very instructive one, in which the artificial differ from the real crystals. If we cut a plate of calcareous spar into any number of pieces, each piece, however minute, will produce the system of coloured rings in as great perfection as the whole plate of which it formed a part; and its polarising force will suffer no diminution by the reduction of its size, provided its thickness remain the same. If we subdivide, however, a circular plate of glass, the polarising force of each portion is not only much diminished, and sometimes even destroyed, if the portion is very small, but the polarising influence is distributed in a new manner, according to the outward shape of the fragment.* Hence it follows, that in glass the polarising structure *depends entirely on the external form of the plate*, and on the mode of aggregation of its particles. When its form is circular, it has only one axis of polarisation; and this axis is positive, if the density diminishes towards the centre, and negative, if it increases towards the centre; but when

* See *Phil. Trans.* 1816, pp. 71, 82.

its form is rectangular, or elliptical, it has two axes of polarisation, the strongest of which appears to be positive, and the weakest negative.

In crystals, on the contrary, the polarising structure is wholly independent of their outward form, and of any variation of density in the aggregation of their particles, and must therefore depend on the form of their integrant molecules, and the variation in their density. Is it not more than probable, therefore, that we have in our artificial crystals, a representation of the ultimate particles of crystallized bodies? When these crystals have a spherical form, diminishing in density towards an axis, and have these axes arranged by the laws of crystallization, they will constitute a crystal of the positive class, like *ice*, *zircon*, *quartz*, &c.; and the only difference between ice and water will be, that in the ice the particles have their axes regularly disposed; while in the water, the axes have every possible direction, so as to create a general equilibrium of the polarising forces. When the density of the spheres increases towards their axes, their symmetrical combination will constitute a crystal of the negative class, such as *beryl*, *calcareous spar*, &c.

But if the particles, instead of being spheres, are either prolate or oblate spheroids, having their polarising axis at right angles to the axis of revolution; and if their density varies, as in elliptical plates of glass, along which heat is in the act of being transmitted, they will constitute by their regular combination, the class of crystals with two axes, such as *topaz*, *sulphate of lime*, &c. in which the positive force predominates. If the density, on the other hand, varies as in

elliptical plates heated in oil, and in the act of sudden cooling, the combined spheroids will constitute the class of crystals like *mica*, *nitre*, &c. in which the negative force predominates.

The variation of density which these solids necessarily require, in order to develope a force varying as the fourth power, or an influence upon the tints varying as the square of the sine of the angle which the ray forms with the axes of the crystals, will be understood from Pl. xvi. fig. 14. Let M*a*N*b* represent the section of the sphere through one of its great circles, then if we suppose that the density is a maximum in every point of the line MN, and that it diminishes from M to *ab*, and from L to *dc*, at first very slowly, and then very rapidly, like the cosines of the angles reckoned from MP, so that the lines of equal density in every section of the sphere passing through MN are diameters, the sphere will represent one of the integrant particles of the positive crystals, like *ice*, *zircon*, *quartz*, *&c.* having MN for its axis of double refraction and polarisation. In like manner, if the density is a minimum in the line MN, and increases in a similar manner, towards *ab* and *cd*, the sphere will represent an integrant particle of the negative crystals, such as *beryl*, *calcareous spar*, *&c.* When the polarised ray is transmitted along the axis MN, the polarising force will be nothing; when it passes along RS, the variation of density is greater, and consequently the polarising force will be increased; and when it is transmitted along *ab*, the polarising force will be a maximum, as the variation of the density is a maximum in that line. As the same is true of every other section of the sphere passing through MN, it follows that there will be a

certain rate of variation in the density at which the polarising force will be proportional to the fourth power, or the variable influence upon the tints to the square of the sine of the angle which the polarised ray forms with the axis.

The preceding reasoning must not be considered as mere speculation, for such a crystal as that represented in fig. 14, may be actually constructed with plates of glass. Let ABCD, for example, represent in section, a circular plate of glass whose thickness is ML, and along whose axis MN, is seen the system of coloured rings shown in Pl. xv. fig. 1. Then, if we conceive the plate bent back into the position *a* M *bc* L *d*, we have the external spherical stratum of our elementary sphere; and in like manner we may conceive the interior strata to be formed by a succession of circular plates DEFC, &c. bent into the spherical form *d* L *cf* K *e*. Now, since the tints of the circular plate ABCD vary as the squares of the distances from its axis ML, we may suppose that the same law still exists, after it is bent into *a* M *bc* L *d*. But the distances from the axis MN are now the sines of the angular distances from M; and therefore, since the same is true of all the other spherical strata, of which the elementary sphere is composed, it follows that the tints produced by the transmission of polarised light, along any diameter of the sphere, are proportional to the square of the sine of the angle, which the ray forms with the axis of the crystal. In order to construct this sphere artificially, we have only to crystallize a series of hemispherical strata, and join them together at the line *ab* in the manner represented in the figure. This sphere, it will be readily seen, is totally different from a solid sphere of crystallized glass, which has no particular axis, but which gives

the same phenomena by transmitting polarised light through any of its diameters.*

In the very same manner we may suppose the elementary spheroid of crystals with two axes to be formed by elliptical plates, bent into spheroidal strata, and the spheroid itself may be actually constructed by means of spheroidal strata of glass. When this is done, it will exhibit all the complicated phenomena produced by the simultaneous actions of two unequal axes.

SECTION VII. *On the laws of double refraction in crystals with any number of axes.*

From the coincidence which I have invariably observed between the axes of polarisation and extraordinary refraction, in all crystals that have only one apparent axis, we may consider it as an established fact, that the HUYGENIAN law represents, within the limits of experimental errors, not only the phenomena of calcareous spar, but those of all other crystals with a single axis of double refraction.

But since it is the general character of crystals to have more than one axis of polarisation, it becomes interesting to ascertain, if they also have two axes of extraordinary refraction; and if they have, to investigate, by direct experiment, the general laws of double refraction, for crystals with any number of axes. Such an investigation presents difficulties of no ordinary kind. The want of a transparent mineral with two powerful axes, which, like calcareous spar, could be obtained in large pieces, and cut with facility in every direc-

* See *Phil. Trans.* 1816, pp. 314, 315.

tion; and the necessity that it should have its resultant axes considerably inclined to each other, in order to obtain a measurable separation of the images at several points, between these resultant axes, rendered all my experiments for a long time completely unsuccessful. The discovery, however, of crystals which possessed, in some degree, the most important of these requisites, has enabled me to resume and to complete the investigation.

After ascertaining the position of the diameters of no polarisation in a crystal with two axes, I formed a prism with a great refracting angle, so as to have a flat surface as perpendicular as possible to one of these diameters. I then placed this prism upon the goniometer, and having marked the position when the point of no polarisation, or the centre of the rings (namely P, Plate xv. fig. 4.) was coincident with the polarised image of a candle, I substituted the direct image of a candle, and observed that this image was single, and therefore that the force of double refraction, as well as the polarising force, had completely vanished. Upon turning the goniometer to both sides of these positions, the deviation of the extraordinary ray became perceptible and gradually increased; and in continuing to observe this deviation in the plane COD passing through the resultant axes, I found that it increased to the middle point O between these axes, where it became stationary; and that it again gradually diminished towards the other resultant axis P' where the image again became single. I now measured the deviation of the extraordinary ray at the points O, A, C, and at various other points, both in and out of these rectangular directions, and I found that the force of double

refraction, varied in the same ratio as the polarising force, and that all the phenomena, whatever be the number of axes by which they are produced, may be calculated by the same general law which we have already established for the phenomena of polarisation.

Let it be required, for example, to determine the velocity of the extraordinary ray in a crystal with any number of positive and negative axes. By the principles explained in Sect. IV. these axes may be reduced to two equivalent rectangular axes, which may be either of the same, or of opposite names. Let us then take $b =$ the axis of revolution of the two spheroids, a, a' the other axis, β, β' the inclination of the incident ray to the axes of the crystal, ψ the angle of the forces as found in Sect. III. and ζ half the difference of the angles at the base of the parallelogram of forces. Then since the velocity of the light is inversely as the variable radius of the spheroid $\frac{1}{b^2}$ will be the square of the velocity of the ordinary ray, and $\frac{1}{a^2}$ $\frac{1}{a'^2}$ the square of the minimum velocity of the extraordinary ray in virtue of the separate action of each axis. The difference between the squares of the velocities of the ordinary and extraordinary rays will be

$$\left(\frac{a^2 \pm b^2}{a^2 b^2}\right) \text{Sin.}^2 \beta$$

$$\left(\frac{a'^2 \pm b^2}{a'^2 b^2}\right) \text{Sin.}^2 \beta'$$

the sign being positive, when the axis is positive, and vice versa. But as these expressions represent the sides of the parallelogram of forces, we have

$$\text{Tang. } \zeta = \frac{\left(\frac{a^2 \pm b^2}{a^2 b^2} \text{Sin.}^2 \beta - \left(\frac{a'^2 \pm b^2}{a'^2 b^2} \text{Sin.}^2 \beta'\right) \text{Tang } \frac{1}{2} \psi\right.}{\frac{a^2 \pm b^2}{a^2 b^2} \text{Sin.}^2 \beta + \frac{a'^2 \pm b^2}{a'^2 b^2} \text{Sin.}^2 \beta'}$$

Consequently the difference between the squares of the velocities of the ordinary and extraordinary ray produced by the combined action of the two axes will be

$$\frac{\left(\frac{a^2 \pm b^2}{a^2 b^2} \text{Sin.}^2\beta\right)(\text{Sin. } \psi)}{\text{Sin. } (\zeta + \frac{1}{2}\psi)}$$

Hence, calling V the velocity required, we have

$$V^2 = \frac{1}{b^2} \pm \frac{\left(\frac{a^2 \pm b^2}{a^2 b^2} \text{Sin.}^2\beta\right)(\text{Sin. } \psi)}{\text{Sin. } (\zeta + \frac{1}{2}\psi)} \text{ and}$$

$$V = \left(\frac{1}{b^2} \pm \frac{\left(\frac{a^2 \pm b^2}{a^2 b^2} \text{Sin.}^2\beta\right)(\text{Sin. } \psi)}{\text{Sin. } (\zeta + \frac{1}{2}\psi)}\right)^{\frac{1}{2}}$$

The form of the compound, or irregular spheroid, may therefore be computed for all doubly refracting crystals.

The general law of double refraction which has now been explained, may be thus expressed.

The increment of the square of the velocity of the extraordinary ray produced by the action of two axes of double refraction, is equal to the diagonal of a parallelogram whose sides are the increments of the square of the velocity produced by each axis separately, and calculated by the law of HUYGENS, *and whose angle is double of the angle formed by the two planes passing through the ray and the respective axes.*

When the two axes are of equal intensity and of the same character, the preceding law gives the very same results as the law of HUYGENS does for one axis placed at right angles to the other two.

It is scarcely necessary to observe, to those who have studied the preceding sections, that the phenomena of double refraction cannot be referred to the *ordinary* action of at-

tractive and repulsive forces. If a ray of light is exposed to the ordinary action of a repulsive or an attractive force emanating from two equal and rectangular axes, there is no point of incidence at which the resultant of these forces is nothing, or there is no resultant axis along which the ray ceases to be divided. In like manner, if the ray is exposed to the action of two equal and rectangular axes, one of which is repulsive and the other attractive, there are innumerable points of incidence in which the resultant of the forces is nothing, and these points are situated in the circumference of two great circles perpendicular to the plane passing through the axes, and bisecting the right angle which they form with each other. These, however, are results entirely incompatible with the actual phenomena of double refraction.

In the preceding sections of this letter, I have included only the more general results of my researches, and have reserved, for another paper, an account of my experiments on the absolute polarising forces of crystals; on the form of the ellipsoids by which their double refraction is regulated, and on the position of their axes with respect to their natural faces, or with respect to certain fixed lines in their primitive forms.

In treating of the coloured rings produced by polarised light, I have likewise omitted the phenomena which are peculiar to individual crystals; and I have not ventured to adopt any theory of their formation.

I cannot, however, conclude this paper without noticing the happy application which Dr. THOMAS YOUNG has made of his beautiful law of interference to the explanation of

this class of colours. The various phenomena of thick and thin plates; the colours of inflected light; the hues of the supernumerary rainbow; the fringes which I observed with two inclined plates of glass of equal thickness; the colours noticed by Mr. NICHOLSON and Mr. KNOX, with plates of unequal thickness; the communicable colours of mother of pearl and striated surfaces; and the colours which I have discovered by the successive reflection of polarised light between metallic plates, and surfaces that produce total reflection, are all referable to this simple law. But while we thus admire the wide range of phenomena which the law of interference embraces, it is necessary also to state, that its application to the colours of polarised light is still attended with some difficulties, and that there are other phenomena of complementary colours which I have lately observed, that resist this method of classification. These, however, are merely difficulties in the application of the law, and not objections to its generality; and I have no doubt that Dr. YOUNG will succeed in referring them to the same cause, and will thus add to the honour which already belongs to him, of having generalised a long train of perplexing and important phenomena.*

I have the honour to be, &c. &c.

DAVID BREWSTER.

To the Right Hon.
Sir Joseph Banks, Bart. &c. &c. &c.

* Having communicated to Dr. YOUNG the contents of the preceding paper, he has requested me to subjoin to it the following letter:

"My Dear Sir,

"Your experiments, on the colours afforded by crystals having two optical axes, appear to establish a very important result in the theory of light; for

Fig. 1.

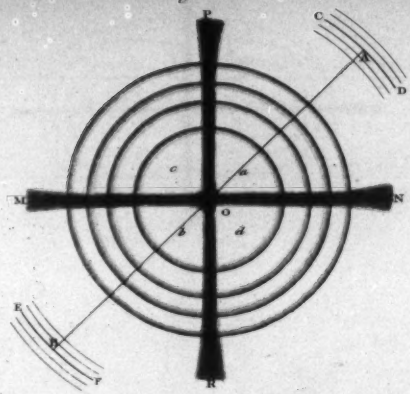


Fig. 2.

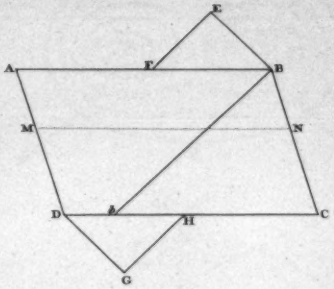


Fig. 3.

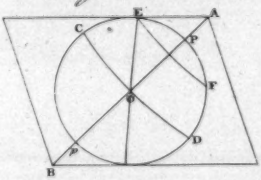


Fig. 6.

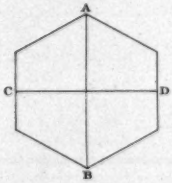


Fig. 4.

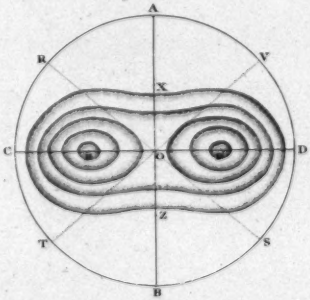


Fig. 5.

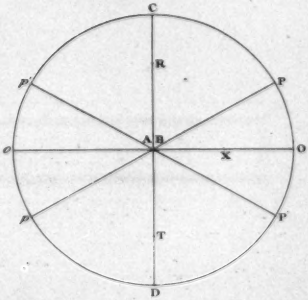


Fig. 7.

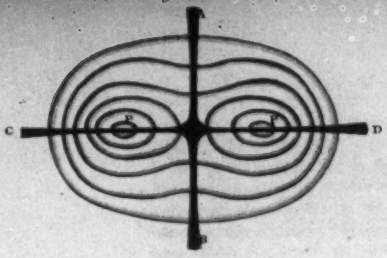


Fig. 8.

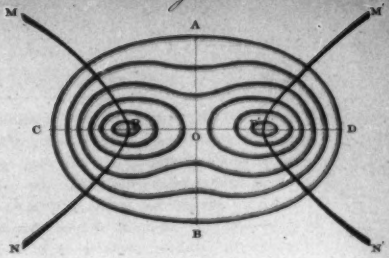


Fig. 10.

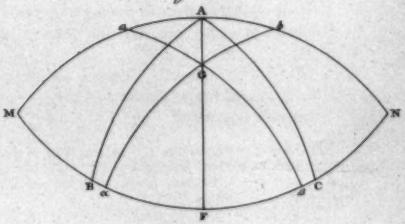


Fig. 9.

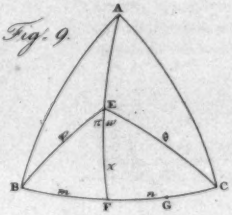


Fig. 12.

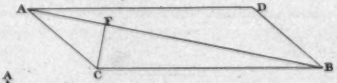


Fig. 11.

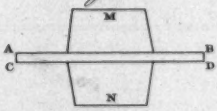


Fig. 14.

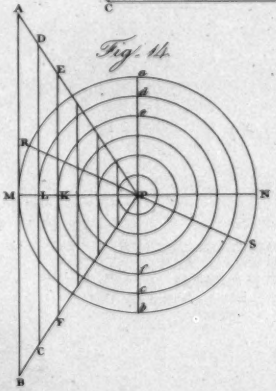
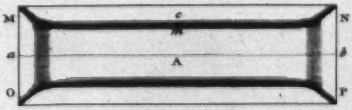
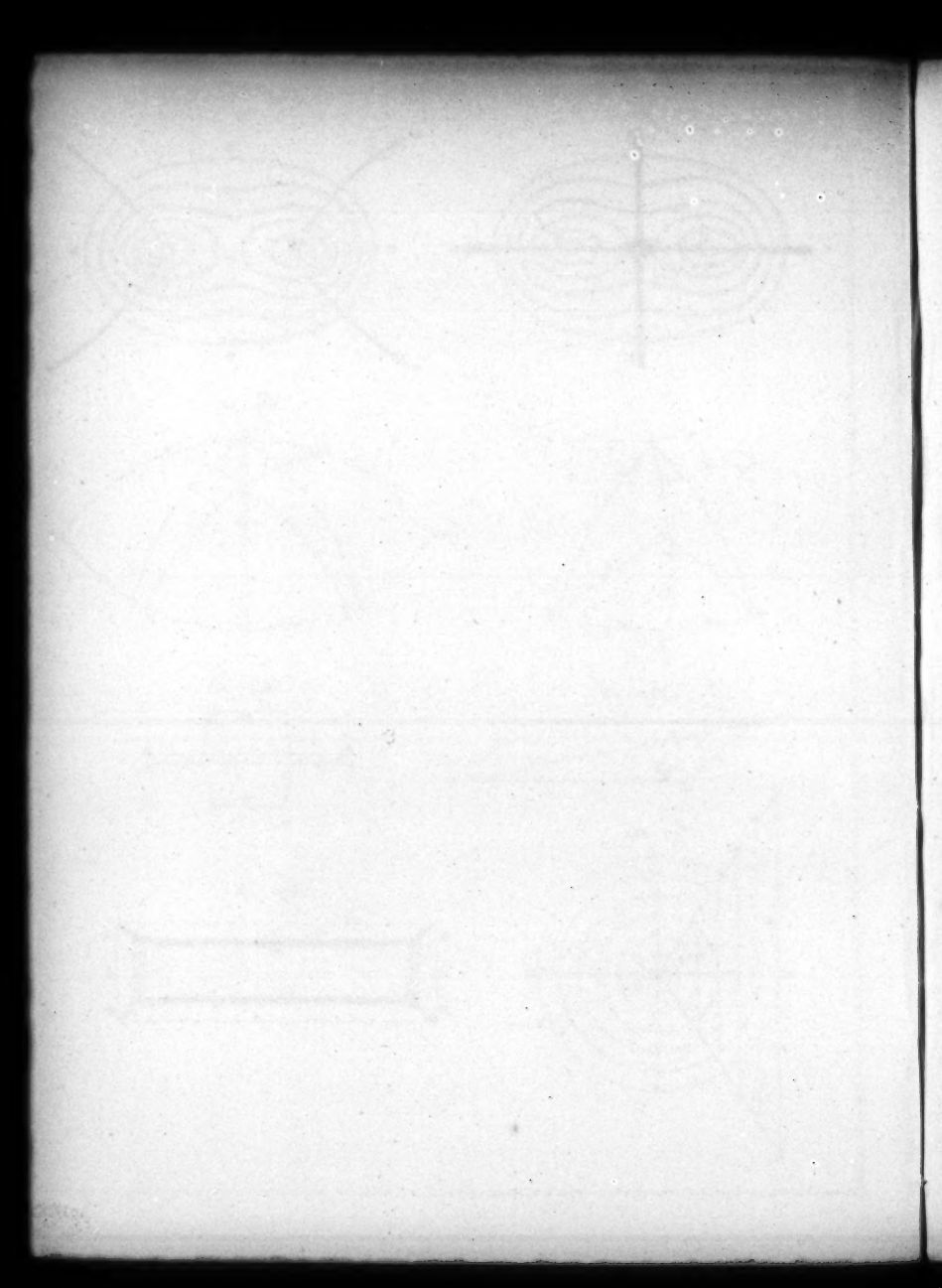


Fig. 13.





supposing them to be perfectly represented by your general law, it will follow that the tint exhibited depends not on the difference of refractive densities in the direction of the ray transmitted, but on the greatest difference of refractive densities in directions perpendicular to that of the ray. These two conditions lead to the same result, where the effect of one axis only is considered, but they vary materially where two axes are supposed to be combined; and I do not immediately perceive by what modification it will be possible to accommodate the laws of interference to these experiments. There can be little doubt that the direction of the polarisation, in such cases, must be determined by that of the greatest and least of the refractive densities in question †; and it seems to be very possible to apply your mode of calculation to many other phenomena, in which the polarising powers of different crystals are combined.

Believe me, dear Sir,

Your faithful and obedient servant,

THOMAS YOUNG."

† This supposition of Dr. YOUNG's is quite correct. In another paper, which will soon be submitted to the Royal Society, I have given a general method of finding the direction of the polarisation for any combination of axes.

From the Press of
W. BULMER & Co.
Cleveland-row, St. James's,
London.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

MDCCCXVIII.

METEOROLOGICAL JOURNAL
for January, 1817.

1817	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Winds. Str.	Weather.
Jan. 1	8	0	43	51	29,56		S	1,2	Fair.
	2	0	47	50	29,45		S	1,2	Rain.
2	8	0	42	50	29,41		S	1	Fine.
	2	0	46	55	29,50		S	1	Fine.
3	8	0	37	50	29,57		W	1	Fine, much wind in the night.
	2	0	42	52	29,62		W	1	Fine.
4	8	0	47	50	29,27		SW	2,3	Rain.
	2	0	51	54	29,27		SW	2	Cloudy.
5	8	0	40	50	29,82		NW	1,2	Fine.
	2	0	45	51	29,79		W	1	Rain. [in the night.
6	8	0	42	48	29,57		S	2	Cloudy, much wind and rain
	2	0	43	52	29,55		W	1	Cloudy.
7	8	0	35	47	30,27		W	1	Cloudy.
	2	0	40	51	30,43		N	1	Fine.
8	8	0	31	48	30,44		NW	1	Thick Fog.
	2	0	41	51	30,44		NW	1	Cloudy.
9	8	0	35	46	30,56		SSE	1	Fine.
	2	0	38	50	30,58		S	1	Foggy.
10	8	0	29	47	30,61		W	1	Foggy.
	2	0	28	46	30,55		W	1	Cloudy.
11	8	0	30	44	30,43		W	1	Cloudy thick weather.
	2	0	34	37	30,36		NNW	1	Cloudy.
12	8	0	32	43	30,15		W	1	Cloudy.
	2	0	38	44	30,04		NNW	1	Cloudy.
13	8	0	34	42	29,78		W	1	Cloudy and hazy.
	2	0	38	46	29,64		SW	1	Cloudy.
14	8	0	35	43	29,54		NW	1	Fine.
	2	0	38	48	29,57		W	1	Fine.
15	8	0	33	44	28,95		E	1	Rain, snow in the night.
	2	0	34	46	28,89		S	1	Snow.
16	8	0	31	42	29,34		N	1	Cloudy.
	2	0	37	48	29,00		S	2	Rain.

Rain this Month 2,154 Inches.

METEOROLOGICAL JOURNAL

for January, 1817.

1817	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Winds. Str.	Weather.
Jan. 17	8	0	41	46	28,88		S	1	Cloudy.
	2	0	44	48	28,89		S	1	Cloudy.
18	8	0	43	48	29,06		S	1	Fine.
	2	0	44	52	29,09		S	1	Cloudy.
19	8	0	41	48	29,07		E	1	Cloudy.
	2	0	43	48	28,86		E	1	Cloudy.
20	8	0	47	47	28,80		SSE	2,3	Fine.
	2	0	48	51	28,82		S	3	Cloudy.
21	8	0	37	48	29,49		W	1	Cloudy.
	2	0	42	54	29,70		W	1	Fine.
22	8	0	42	49	29,77		W	2	Rain.
	2	0	47	53	29,86		W	1,2	Cloudy.
23	8	0	41	51	29,88		W	1	Cloudy.
	2	0	53	56	30,02		W	1	Cloudy.
24	8	0	48	54	30,20		S	1	Cloudy.
	2	0	49	57	30,24		W	1	Cloudy.
25	8	0	48	54	30,31		W	1	Cloudy.
	2	0	49	56	30,20		SW	1	Cloudy.
26	8	0	44	53	30,25		S	1	Cloudy.
	2	0	45	58	30,19		W	1	Cloudy.
27	8	0	47	52	30,31		W	1	Cloudy, thick weather.
	2	0	49	54	30,35		E	1	Cloudy.
28	8	0	43	52	30,49		W	1	Dark and Cloudy.
	2	0	45	56	30,34		S	1	Cloudy.
29	8	0	44	51	30,30		W	1	Cloudy.
	2	0	47	57	30,33		NW	1	Cloudy.
30	8	0	43	52	30,33		W	1	Cloudy.
	2	0	50	56	30,28		E	1	Cloudy.
31	8	0	43	53	30,43		N	1	Cloudy.
	2	0	49	57	30,47		N	1	Cloudy.

Rain this Month 2,154 Inches.

METEOROLOGICAL JOURNAL

for February, 1817.

1817	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Str.	Weather.
Feb. 1	8	0	40	52	30.53		N	1	Cloudy and hazy.
	2	0	50	57	30.53		W	1	Cloudy.
2	8	0	42	52	30,50		W	1	Cloudy.
	2	0	44	52	30.44		W	1	Cloudy.
3	8	0	38	49	30,35		W	1	Cloudy.
	2	0	42	53	30.27		NW	1	Cloudy.
4	8	0	40	50	30,01		SW	1	Cloudy.
	2	0	43	55	29,76		SW	1	Cloudy.
5	8	0	39	49	29,73		NW	1	Fine.
	2	0	45	54	29,90		NW	1	Cloudy.
6	8	0	49	51	30,01		W	1	Cloudy.
	2	0	50	57	30,06		W	1	Cloudy.
7	8	0	45	53	30,17		W	1	Hazy.
	2	0	52	58	30,26		WNW	1,2	Cloudy.
8	8	0	46	54	30,28		W	1	Cloudy.
	2	0	46	55	30,33		W	1	Cloudy.
9	8	0	49	54	30,35		W	1	Cloudy.
	2	0	49	52	30,36		W	1	Cloudy.
10	8	0	48	52	30,18		W	1	Cloudy.
	2	0	49	55	30,07		W	1,2	Cloudy.
11	8	0	42	53	29,84		N	1	Rain.
	2	0	43	56	30,00		N	1	Fine.
12	8	0	40	51	29,47		W	1	Cloudy.
	2	0	45	53	29,61		N	1	Cloudy.
13	8	0	40	50	29,85		W	1	Rain.
	2	0	50	57	29,79		W	1	Rain. [night.
14	8	0	41	53	29,50		W	1	Cloudy, rainy, and stormy
	2	0	43	57	29,70		N	1	Fine.

Rain this Month 0,875 Inches.

METEOROLOGICAL JOURNAL
for February, 1817.

1817	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Winds. Points.	Str.	Weather.
Feb. 15	8	0	40	53	29.53	W	1	Rain.
	2	0	50	56	29,52	NW	1	Fine.
16	8	0	41	51	29,70	NNW	1	Fine.
	2	0	49	52	29,83	NW	1	Fine.
17	8	0	46	50	29,96	W	1	Rain.
	2	0	53	54	29,97	W	1	Fine.
18	8	0	48	53	30,11	W	1	Cloudy.
	2	0	51	56	30,01	W	1	Cloudy.
19	8	0	42	53	30,18	NNW	1	Hazy.
	2	0	47	58	30,24	S	1	Fair.
20	8	0	44	52	29,85	S	1,2	Cloudy, hard gale of wind in
	2	0	49	57	29,51	S	1	Cloudy. [the night.
21	8	0	37	52	29,56	W	1	Fine.
	2	0	44	55	29,52	WbyN	1	Fine.
22	8	0	40	52	29,62	N	1	Fine.
	2	0	46	57	29,81	NW	1	Cloudy.
23	8	0	40	52	29,98	W	1	Cloudy.
	2	0	49	52	29,82	S	1	Cloudy.
24	8	0	43	51	29,77	W	1	Cloudy.
	2	0	47	55	29,91	W	1	Cloudy.
25	8	0	42	52	30,01	W	1	Fine.
	2	0	50	57	29,98	W	1	Cloudy.
26	8	0	42	53	29,73	N	1	Fair.
	2	0	48	57	29,88	WNW	1	Cloudy.
27	8	0	47	53	29,60	W	1	Fine.
	2	0	50	55	29,66	NW	1,2	Fine.
28	8	0	46	54	29,88	W	1	Cloudy.
	2	0	48	60	29,87	W	1	Fair.

Rain this Month 0,875 Inches.

METEOROLOGICAL JOURNAL
for March, 1817.

1817	Time. H. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Winds. Points.	Str.	Weather
Mar. 1	7 0	47	56	29.77	W	1	Cloudy.
	2 0	52	59	29.57	SW	1	Cloudy.
2	7 0	39	53	29.74	W	1	Cloudy.
	2 0	48	55	29.66	W	1	Fine.
3	7 0	39	51	29.44	SW	1	Fine, a stormy night.
	2 0	43	54	29,06	W	1,2	Rain.
4	7 0	40	51	29.22	W	1	Fine.
	2 0	42	56	29.21	W	1	Fine.
5	7 0	36	50	29.27	E	1	Fair.
	2 0	48	56	29.35	NW	1	Fair.
6	7 0	40	52	28,87	W	1	Rain.
	2 0	44	56	29.07	NW	1	Fine.
7	7 0	36	50	29.26	W	1	Fine.
	2 0	47	56	29.35	NW	1	Fine.
8	7 0	38	52	29.07	W	1	Rain.
	2 0	49	56	29.24	W	1	Fine, a fall of snow at 10 a. m.
9	7 0	37	49	29.46	W	1	Cloudy.
	2 0	47	53	29,61	W	2	Fine.
10	7 0	36	48	29.98	W	1	Fair.
	2 0	52	55	30,06	WNW	1	Fine.
11	7 0	37	49	30,17	W	1	Fine.
	2 0	48	53	30,17	W	1	Cloudy.
12	7 0	44	52	30,02	S	1	Fine.
	2 0	52	56	29.94	W	1	Cloudy.
13	7 0	48	54	29.98	N	1	Cloudy and hazy.
	2 0	53	57	30,01	WNW	1	Cloudy.
14	7 0	45	54	30,22	E	1	Fine.
	2 0	55	61	30,24	E	1	Fine.
15	7 0	40	54	30,28	N	1	Cloudy.
	2 0	49	58	30,29	N	1	Cloudy.
16	7 0	39	52	30,24	SSE	1	Cloudy.
	2 0	46	57	30,20	E	1	Fine.

Rain this Month 1,057 Inches.

METEOROLOGICAL JOURNAL
for March, 1817.

1817	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Winds. Points.	Scr.	Weather.
Mar. 17	7	0	38	51	30,27	N	1	Cloudy.
	2	0	45	55	30,31	E	1	Fine.
18	7	0	37	51	30,28	WSW	1	Fine.
	2	0	50	56	30,15	SW	1	Fine.
19	7	0	45	53	29,92	W	1	Fine.
	2	0	51	57	29,90	N	1	Fine.
20	7	0	34	51	29,79	NW	1	Fine.
	2	0	38	53	29,85	N	1	Cloudy.
21	7	0	30	47	29,93	N	1	Fine.
	2	0	39	54	29,94	N	1	Fine.
22	7	0	30	48	29,94	N	1	Hazy.
	2	0	40	54	29,97	N	1	Fine.
23	7	0	31	50	30,05	E	1	Fine.
	2	0	44	55	30,03	S	1	Fine.
24	7	0	44	49	29,95	SW	1	Cloudy.
	2	0	49	54	29,87	SW	1	Cloudy.
25	7	0	46	51	29,79	W	1	Rain.
	2	0	50	56	29,89	NE	1	Fine.
26	7	0	44	52	29,95	W	1	Cloudy.
	2	0	50	56	29,84	W	1	Cloudy.
27	7	0	39	52	30,03	W	1	Fine.
	2	0	46	58	30,13	W	1	Fine.
28	7	0	39	51	30,00	S	1	Cloudy.
	2	0	48	53	29,84	W	1	Cloudy.
29	7	0	45	50	29,90	W	1	Cloudy.
	2	0	50	54	29,99	W	1	Cloudy.
30	7	0	48	55	30,05	W	1	Fair.
	2	0	56	58	30,10	N	1	Fine.
31	7	0	44	53	30,25	NW	1	Cloudy.
	2	0	52	59	30,50	N	1	Fine.

Rain this Month 1,057 Inches.

METEOROLOGICAL JOURNAL

for April, 1817.

1817	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Winds. Points.	Winds. Str.	Weather.
Apr. 1	7	0	45	53	30,56	S	1	Fair.
	2	0	53	63	30,54	E	1	Fair.
2	7	0	43	55	30,45	E	1	Thick and foggy.
	2	0	53	63	30,38	E	1	Fair.
3	7	0	43	57	30,35	N	1	Fine, but rather hazy.
	2	0	54	62	30,35	N	1	Fine.
4	7	0	45	57	30,42	N	1	Fine.
	2	0	52	64	30,40	E	1	Fine.
5	7	0	43	56	30,38	E	1	Dark and hazy.
	2	0	50	62	30,37	N	1	Fine.
6	7	0	40	54	30,38	N	1	Fair.
	2	0	47	55	30,43	N	1	Cloudy.
7	7	0	44	53	30,49	N	1	Cloudy.
	2	0	50	61	30,47	E	1	Fine.
8	7	0	40	53	30,27	W	1	Hazy.
	2	0	53	62	30,04	W	1	Fine.
9	7	0	44	54	29,95	SW	1	Fine, rain in the night.
	2	0	50	57	30,04	NE	1	Cloudy.
10	7	0	36	52	30,03	N	1	Fair.
	2	0	40	56	30,13	N	1	Fine.
11	7	0	34	50	30,30	N	1	Cloudy.
	2	0	44	55	30,30	N	1	Cloudy.
12	7	0	42	52	30,12	W	1	Cloudy.
	2	0	59	55	30,06	N	1	Cloudy.
13	7	0	46	53	30,08	NW	1	Cloudy.
	2	0	52	53	30,12	N	1	Cloudy.
14	7	0	48	52	30,09	N	1	Fine, rather hazy.
	2	0	51	59	30,06	N	1	Fine.
15	7	0	53	55	29,99	N	1	Cloudy.
	2	0	58	60	29,96	N	1	Fine.

Rain this Month 0,120 Inches.

METEOROLOGICAL JOURNAL

for April, 1817.

1817	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Winds. Str.	Weather.
Apr. 16	7	0	52	56	29,75		N	1	Cloudy.
	2	0	50	59	29,95		N	1,2	Fine.
17	7	0	40	52	30,17		N	1,2	Fine.
	2	0	45	55	30,25		N	1	Cloudy.
18	7	0	40	52	30,39		N	1	Cloudy.
	2	0	49	57	30,43		N	1	Cloudy.
19	7	0	40	53	30,45		NW	1	Cloudy.
	2	0	52	58	30,43		NE	1	Fine.
20	7	0	48	55	30,43		N	1	Fair.
	2	0	55	60	30,44		N	1	Fine.
21	7	0	45	54	30,43		N	1	Fine.
	2	0	48	62	30,38		N	1	Fine.
22	7	0	44	55	30,36		NW	1	Cloudy.
	2	0	52	63	30,31		NE	1	Fine.
23	7	0	43	55	30,27		NE	1	Cloudy.
	2	0	49	57	30,24		E	1	Rain.
24	7	0	41	53	30,28		SSE	1	Cloudy.
	2	0	53	58	30,27		N	1	Fine.
25	7	0	42	53	30,32		N	1	Cloudy.
	2	0	42	56	30,27		N	1	Cloudy.
26	7	0	42	53	30,13		SE	1	Cloudy.
	2	0	48	56	30,09		N	1	Cloudy.
27	7	0	43	54	29,99		SE	1,2	Cloudy.
	2	0	51	55	30,07		N	1	Fair.
28	7	0	43	50	30,18		W	1	Cloudy.
	2	0	53	54	30,14		W	1	Cloudy.
29	7	0	46	48	30,01		W	1	Cloudy.
	2	0	53	57	29,89		W	1	Cloudy.
30	7	0	44	54	29,76		W	1	Cloudy.
	2	0	48	59	29,78		N	1	Fine.

Rain this Month 0,120 Inches.

METEOROLOGICAL JOURNAL
for May, 1817.

1817	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Str.	Weather.
May 1	7	0	45	54	29,87		N	1	Cloudy.
	2	0	49	57	29,92		N	1	Cloudy.
2	7	0	42	54	30,00		N	1	Cloudy.
	2	0	53	60	30,00		N	1	Cloudy.
3	7	0	44	55	29,95		WNW	1	Cloudy.
	2	0	55	60	29,85		N	1	Cloudy.
4	7	0	50	56	29,84		W	1	Cloudy.
	2	0	57	59	29,92		W	1	Fine.
5	7	0	48	54	30,19		S	1	Fine.
	2	0	58	62	30,07		W	1	Fair.
6	7	0	50	57	30,14		W	1	Fine.
	2	0	58	61	30,19		N	1	Fine.
7	7	0	48	56	30,29		E	1	Fine.
	2	0	55	61	30,14		E	1	Fine.
8	7	0	52	57	29,88		NE	1	Hazy.
	2	0	63	62	29,81		E	1	Cloudy.
9	7	0	48	58	29,81		E	1	Cloudy.
	2	0	52	61	29,76		NE	1	Cloudy.
10	7	0	49	57	29,60		S	1	Cloudy.
	2	0	58	62	29,49		S	1	Fine.
11	7	0	47	57	29,50		NW	1	Hazy.
	2	0	57	60	29,54		NW	1	Fine.
12	7	0	48	55	29,34		W	1	Cloudy.
	2	0	58	62	29,42		E	1	Fine.
13	7	0	46	55	29,71		W	1	Fine.
	2	0	51	58	29,78		SSW	1	Fine.
14	7	0	42	54	29,69		SE	1	Rain.
	2	0	50	56	29,57		N	1	Fine.
15	7	0	47	55	29,78		W	1	Cloudy.
	2	0	57	58	29,88		N	1	Fine.
16	7	0	49	57	29,98		SW	1	Fair.
	2	0	64	64	29,94		E	1	Fine.

Rain this Month 1,698 Inches.

METEOROLOGICAL JOURNAL
for May, 1817.

1817	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Str.	Weather.
May 17	7	0	51	57	29,89		W	1	Showery.
	2	0	61	62	29,83		W	1	Cloudy.
18	7	0	54	59	29.69		E	1	Cloudy.
	2	0	65	62	29.55		E	1	Rain.
19	7	0	50	58	29.52		NE	1	Cloudy.
	2	0	53	60	29.54		N	1	Rain.
20	7	0	45	57	29.51		N	1	Rain.
	2	0	52	59	29.51		N	1	Cloudy.
21	7	0	47	57	29.44		NW	1	Rain.
	2	0	42	59	29.48		W	1.2	Rain.
22	7	0	44	55	29.48		W	1	Cloudy.
	2	0	50	59	29.49		W	1	Fair.
23	7	0	46	56	29.52		SW	1	Cloudy.
	2	0	57	62	29.51		SW	1	Cloudy.
24	7	0	46	55	29.52		W	1	Fine.
	2	0	58	59	29.52		W	1	Fine.
25	7	0	42	57	29.37		E	1	Rain.
	2	0	52	55	29.28		W	1	Rain.
26	7	0	49	56	29.23		E	1	Cloudy.
	2	0	59	60	29.27		E	1	Cloudy.
27	7	0	52	57	29.54		E	1	Cloudy.
	2	0	64	60	29.52		ESE	1	Fair.
28	7	0	49	56	29.66		N	1	Fair.
	2	0	54	60	29.71		SW	1	Cloudy.
29	7	0	50	58	29.65		NE	1	Cloudy.
	2	0	50	60	29.69		N	1.2	Cloudy.
30	7	0	47	56	29.87		N	1	Cloudy.
	2	0	51	58	29.91		N	1	Cloudy.
31	7	0	45	54	29.95		N	1	Fine.
	2	0	57	58	29,87		N	1	Cloudy.

Rain this Month 1.698 Inches.

METEOROLOGICAL JOURNAL

for June, 1817.

1817	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain this Month 1,860 Inches.	Winds. Points.	Winds. Str.	Weather.
June 1	7	0	47	55	29,78		NW	1	Cloudy.
	2	0	57	56	29,79		N	1	Cloudy.
2	7	0	53	55	29,78		SW	1	Fine.
	2	0	56	57	29,75		W	1	Cloudy.
3	7	0	53	55	29,70		W	1	Cloudy.
	2	0	60	61	29,74		SW	1.2	Fine.
4	7	0	48	58	29,52		W	1.2	Rain.
	2	0	62	61	29,76		N	2	Fine.
5	7	0	53	57	30,10		W	1	Cloudy.
	2	0	59	60	30,11		SW	1	Fair.
6	7	0	54	59	30,03		W	1	Cloudy.
	2	0	66	64	30,03		W	1	Fine.
7	7	0	59	61	29,95		W	1	Cloudy.
	2	0	67	67	29,87		S	1	Fine.
8	7	0	57	62	29,74		W	1	Cloudy.
	2	0	62	65	29,78		NW	1	Cloudy.
9	7	0	55	60	29,88		W	1	Cloudy.
	2	0	54	62	29,82		SW	1	Rain.
10	7	0	56	60	29,75		W	1.2	Cloudy.
	2	0	64	63	29,84		WNW	1	Fine.
11	7	0	52	59	30,06		W	1	Fine.
	2	0	62	62	30,02		W	1	Fine.
12	7	0	55	60	29,80		S	1	Cloudy, rain in the night.
	2	0	56	61	29,68		SW	1	Cloudy.
13	7	0	53	59	29,65		E	1	Cloudy.
	2	0	60	61	29,38		S	2	Rain.
14	7	0	53	59	29,42		W	2	Fine.
	2	0	58	60	29,61		W	1.2	Fine.
15	7	0	53	58	30,12		Nby W	1	Fine.
	2	0	60	59	30,23		NW	1	Fine.

METEOROLOGICAL JOURNAL
for June, 1817.

1817	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Winds. Points.	Winds. Str.	Weather.
June16	7	0	52	58	30,32	WSW	1	Hazy.
	2	0	65	62	30,28	E	1	Fine.
17	7	0	56	59	30,14	E	1	Fine.
	2	0	65	65	30,00	E	1	Fine.
18	7	0	61	60	29,75	E	1	Cloudy and hazy.
	2	0	71	69	29,69	E	1	Fine.
19	7	0	67	63	29,70	SW	1	Fine.
	2	0	77	70	29,72	SW	3	Fine, with showers.
20	7	0	68	66	29 79	E	1	Hazy.
	2	0	79	75	29,79	SE	1	Fair.
21	7	0	69	69	29,87	N	1	Fine.
	2	0	81	77	29,89	SE	1	Fine.
22	7	0	70	71	30,06	NNW	1	Fine.
	2	0	80	77	30,05	NNE	1	Fair.
23	7	0	68	70	30,01	N	1	Fair.
	2	0	78	76	29,96	N	1	Fine.
24	7	0	67	70	29,93	NNW	1	Thick and cloudy.
	2	0	77	77	29,93	WNW	1	Fine.
25	7	0	67	70	29,9	W	1	Fine.
	2	0	74	77	29,96	W	1	Fine.
26	7	0	63	67	29,87	SW	1	Cloudy.
	2	0	74	74	29,79	SW	1	Fine.
27	7	0	69	70	29,74	E	1	Thick and hazy.
	2	0	75	74	29,59	E	1	Fine.
28	7	0	64	66	29,70	W	1	Cloudy.
	2	0	70	71	29,79	W	1	Fine.
29	7	0	59	67	29,96	S	1	Cloudy.
	2	0	68	69	29,91	SSE	1	Fine.
30	7	0	61	66	29,71	W	1	Cloudy.
	2	0	67	69	29,75	NW	1	Fine.

Rain this Month 1,863 Inches.

METEOROLOGICAL JOURNAL
for July, 1817.

1817	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Winds. Str.	Weather.
July 1	7	0	58	65	29,80	Rain this Month 1,411 Inches.	E	1	Cloudy.
	2	0	59	66	29,58		S	2	Rain.
2	7	0	57	64	29,61		W	1,2	Cloudy.
	2	0	66	67	29,83		W	1	Cloudy.
3	7	0	57	64	29,94		W	1	Cloudy.
	2	0	62	65	29,88		S	1	Rain.
4	7	0	56	63	29,65		W	1	Cloudy.
	2	0	64	65	29,64		W	1	Fine.
5	7	0	57	60	29,55		W by N	1	Cloudy.
	2	0	64	64	29,60		NW	1	Cloudy.
6	7	0	57	62	29,73		S	1	Cloudy.
	2	0	66	66	29,72		S	1	Fair.
7	7	0	57	62	29,78		W	1	Fine.
	2	0	68	65	29,77		W	1	Cloudy.
8	7	0	58	63	29,84		W	1	Hazy.
	2	0	66	66	29,86		W	1	Cloudy.
9	7	0	60	61	29,88		NNW	1	Cloudy.
	2	0	68	66	29,88		SW	1	Cloudy.
10	7	0	60	63	29,84		SSW	1	Fine.
	2	0	70	68	29,81		SSW	1	Fine.
11	7	0	62	64	29,79		SW	1	Cloudy.
	2	0	67	67	29,79		SW	1	Cloudy.
12	7	0	62	64	29,83		NW	1	Cloudy.
	2	0	63	65	29,94		SW	1	Cloudy.
13	7	0	58	64	29,99		SW	1	Fine.
	2	0	64	64	29,94		SW	1	Cloudy.
14	7	0	55	59	29,74		W	1	Cloudy, rain in the night.
	2	0	62	64	29,61		WNW	1	Cloudy.
15	7	0	55	61	29,27		SW	1	Rain.
	2	0	61	65	29,17		WNW	1	Cloudy.
16	7	0	54	60	29,62		N	1	Fine.
	2	0	61	62	29,71		NNW	1	Fine.

METEOROLOGICAL JOURNAL
for July, 1817.

1817	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Winds. Str.	Weather.
July 17	7	0	56	60	29.85		N	1	Fine.
	2	0	63	63	29.88		NW	1	Fine.
18	7	0	57	61	29.81		WNW	1	Cloudy.
	2	0	63	60	29.82		WNW	1	Fair.
19	7	0	56	60	29.95		WbyN	1	Fine, rather hazy.
	2	0	62	61	29.97		W	1	Cloudy.
20	7	0	57	58	29.98		W	1	Fine.
	2	0	62	60	29.99		WNW	1	Fine.
21	7	0	58	60	29.93		SW	1	Cloudy.
	2	0	66	64	29.88		SW	1,2	Cloudy.
22	7	0	59	62	29.78		SW	1	Rain.
	2	0	65	64	29.81		SW	1	Cloudy.
23	7	0	60	62	29.89		NW	1	Hazy.
	2	0	63	64	29.92		N	1	Cloudy, thunder in the evening.
24	7	0	58	62	30,03		SW	1	Hazy.
	2	0	67	65	30,04		NW	1	Fair.
25	7	0	60	62	30,01		S	2	Fine.
	2	0	66	68	30,00		S	1	Fine.
26	7	0	59	63	29.94		S	1	Cloudy.
	2	0	60	63	29.80		NNW	1	Cloudy.
27	7	0	57	62	29.66		W byN	1	Fine.
	2	0	65	65	29.80		W	1	Fine.
28	7	0	57	57	29.84		W	1	Cloudy.
	2	0	64	64	29.89		SW	1	Fine.
29	7	0	59	61	30,03		W	1	Fine.
	2	0	61	63	29.98		W	1	Cloudy.
30	7	0	58	61	29.80		W	1	Cloudy.
	2	0	60	63	29.80		NNW	1	Cloudy.
31	7	0	57	61	29.77		W	1	Cloudy.
	2	0	60	63	29.83		W	1	Rain.

Rain this Month 1,411 Inches.

METEOROLOGICAL JOURNAL
for August, 1817.

1817	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Str.	Weather.
Aug. 1	7	0	53	61	29,79		W	1	Fair.
	2	0	64	65	29,82		NW	1	Fine.
2	7	0	54	60	29,93		W	1	Fine.
	2	0	65	67	29,94		NW	1	Cloudy.
3	7	0	57	62	29,78		SW	2	Cloudy.
	2	0	55	64	29,73		NW	1	Fine.
4	7	0	57	61	29,80		W	2	Cloudy.
	2	0	67	66	29,76		NNW	1.2	Fine.
5	7	0	58	62	29,96		NNE	1	Cloudy.
	2	0	68	63	30,06		N	1	Fine.
6	7	0	59	62	30,08		SW	1	Fine.
	2	0	67	68	30,06		SW	1	Fine.
7	7	0	58	63	29,96		E	1	Hazy.
	2	0	68	66	29,85		SW	1	Fine.
8	7	0	57	64	29,57		SW	2	Rain.
	2	0	67	65	29,56		W	1.2	Cloudy.
9	7	0	57	63	29,72		W	1.2	Fine.
	2	0	65	67	29,77		W	1	Cloudy.
10	7	0	58	63	29,86		NW	1	Cloudy.
	2	0	63	65	29,85		NW	1	Cloudy.
11	7	0	53	62	29,86		NW	1	Fine.
	4	0	63	64	29,69		SE	1	Cloudy.
12	7	0	55	62	29,42		W	1	Fine.
	4	0	60	63	29,42		S	1	Cloudy.
13	7	0	58	61	29,25		S	1	Cloudy.
	4	0	60	66	29,58		SW	1	Fine.
14	7	0	60	62	29,57		W	1	Cloudy.
	4	0	68	69	29,76		W	1	Rain.
15	7	0	62	61	29,78		N	1	Cloudy.
	4	0	62	67	29,77		NW	1	Cloudy.
16	7	0	61	61	29,76		S	1	Cloudy.
	4	0	59	64	29,79		SSW	1	Cloudy.

Rain this Month 1,458 Inches.

METEOROLOGICAL JOURNAL

for August, 1817.

1817	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Winds. Str.	Weather.
Aug. 17	7	0	60	60	29,75		NW	1	Cloudy.
	4	0	61	66	29,77		W	1	Cloudy.
18	7	0	60	65	29,76		NE	1	Rain.
	4	0	55	60	29,71		E	1	Cloudy.
19	7	0	54	63	29,90		NW	1	Rain.
	4	0	51	65	29,72		N	1	Fair.
20	7	0	56	67	29,81		N	1	Cloudy.
	4	0	61	63	29,84		W	1	Cloudy.
21	7	0	59	65	29,68		N	1	Cloudy.
	4	0	59	68	29,67		N	1	Rain.
22	7	0	60	61	29,60		W	1	Rain.
	4	0	60	68	29,81		N	1	Rain.
23	7	0	65	67	30,01		E	1	Cloudy.
	4	0	64	66	30,07		N	1	Fine.
24	7	0	63	64	29,99		W	1	Fine.
	4	0	62	61	29,70		W	1	Fine.
25	7	0	60	63	29,61		N	1	Fine.
	4	0	60	61	29,00		S	1	Rain.
26	7	0	59	63	29,16		N	1	Rain.
	4	0	53	62	29,10		E	1	Fair.
27	7	0	53	60	29,29		W	1	Rain.
	4	0	50	60	29,30		E	1	Rain.
28	7	0	56	69	29,60		W	1	Rain.
	4	0	57	68	29,63		E	1	Fair.
29	7	0	54	67	29,64		W	1	Fine.
	4	0	55	68	29,71		N	1	Rain.
30	7	0	59	64	29,79		NE	1	Rain.
	4	0	62	65	29,85		N	1	Fine.
31	7	0	60	68	29,81		E	1	Fine.
	4	0	56	67	29,80		SE	1	Fine.

Rain this Month 1,458 Inches.

METEOROLOGICAL JOURNAL

for September, 1817.

1817	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.		Winds. Points.	Winds. Str.	Weather.
			o	o	Inches.				
Sept. 1	7	0	50	53	30,00		N	1	Fine.
	4	0	59	61	30,01		E	1	Fine.
2	7	0	55	56	29,95		N	1	Fine.
	4	0	59	61	29,97		N	1	Fine.
3	7	0	60	63	29,84		E	1	Fine.
	4	0	61	67	29,87		NNE	1	Fine.
4	7	0	67	63	30,01		N	1	Fine.
	4	0	69	62	30,03		NE	1	Fine.
5	7	0	55	60	30,11		N	1	Fine.
	4	0	59	63	30,12		NNE	1	Fine.
6	7	0	55	61	30,01		N	1	Fine.
	4	0	60	67	30,07		NE	1	Fine.
7	7	0	56	61	30,09		E	1	Fair.
	4	0	60	63	30,06		SSE	1	Fair.
8	7	0	56	62	29,99		S	1	Fair.
	4	0	61	63	29,97		S	1	Fair.
9	7	0	58	59	30,00		E	1	Fair.
	4	0	57	60	29,99		E	1	Fair.
10	7	0	59	61	29,97		E	1	Fair.
	4	0	54	59	29,95		W	1	Fair.
11	7	0	57	59	30,01		NW	1	Fine.
	4	0	56	55	30,00		N	1	Cloudy.
12	7	0	57	60	29,96		NNW	1	Cloudy.
	4	0	59	60	29,95		N	1	Cloudy.
13	7	0	54	59	29,94		N	1	Fine.
	4	0	55	65	29,96		N	1	Fine.
14	7	0	54	60	29,87		E	1	Rain.
	4	0	61	67	29,97		NE	1	Rain.
15	7	0	60	63	30,01		N	1	Rain.
	4	0	62	65	30,11		NW	1	Rain.

Rain this Month 0,300 Inches.

METEOROLOGICAL JOURNAL
for September, 1817.

1817	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Str.	Weather.
Sep. 16	7	0	64	69	30,01		N	1,2	Cloudy.
	4	0	56	60	30,10		N	2	Cloudy.
17	7	0	56	59	29,93		E	1	Fine.
	4	0	61	63	29,90		NE	1	Cloudy.
18	7	0	56	60	29,87		E	1	Cloudy.
	4	0	59	63	29,86		E	1	Cloudy.
19	7	0	58	60	29,89		NW	1	Cloudy.
	4	0	56	60	29,97		NW	1	Cloudy.
20	7	0	55	61	30,00		N	1	Fine.
	4	0	57	63	30,01		N	1	Fine.
21	7	0	55	60	29,97		N	1	Fine.
	4	0	53	61	29,95		E	1	Fine.
22	7	0	49	60	29,84		E	1	Fine.
	4	0	53	61	29,87		E	1	Fine.
23	7	0	51	66	29,83		E	1	Fine.
	4	0	56	63	29,85		NE	1	Fine.
24	7	0	55	59	29,97		N	1	Fine.
	4	0	57	58	29,86		NW	1	Cloudy.
25	7	0	60	61	29,53		N	1	Fine.
	4	0	58	60	29,55		N	1	Fine.
26	7	0	57	60	29,22		E	1	Fine.
	4	0	51	59	29,27		W	1	Fine.
27	7	0	59	54	30,00		N	1	Fine.
	4	0	51	55	30,01		NE	1	Fair.
28	7	0	60	66	30,11		E	1	Fine.
	4	0	63	65	30,19		S	1	Fine.
29	7	0	56	54	30,08		E	1	Fair.
	4	0	57	56	30,06		E	1	Fine.
30	7	0	58	55	29,94		S	1	Fine.
	4	0	57	60	30,00		S	1	Fine.

Rain this Month 0,300 Inches.

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1817	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Winds. Points.	Str.	Weather.
Oct. 1	7	0	59	65	29,08	SE^{b}	1	Fair.
	4	0	48	54	29,08	W	1	Fine.
2	7	0	54	55	29,18	N	1	Fine.
	4	0	53	50	30,10	NE	1	Fine.
3	7	0	50	47	30,11	E	1	Fine.
	4	0	52	48	30,09	NE	1	Fine.
4	7	0	53	53	30,25	N	1	Fine.
	4	0	54	53	30,13	N	1	Fine.
5	7	0	57	59	30,01	E	1	Fine.
	4	0	53	56	30,11	W	1	Fine.
6	7	0	50	52	30,30	E	1	Fine.
	4	0	52	54	30,29	E	1	Fine.
7	7	0	53	52	30,16	W	1	Fine.
	4	0	55	53	30,20	W	1	Fine.
8	7	0	52	53	30,14	E	1	Fine.
	4	0	52	54	30,10	S	1	Fine.
9	7	0	52	53	30,02	SE	1	Fine.
	4	0	53	55	30,03	SW	1	Fine.
10	7	0	52	54	29,92	S	1	Cloudy.
	4	0	53	54	29,93	SW	1	Cloudy.
11	7	0	53	56	30,01	SW	1	Cloudy.
	4	0	49	55	30,10	SE	1	Cloudy.
12	8	0	47	52	30,04	SE	1	Fine.
	2	0	50	55	30,05	SW	1	Fine.
13	8	0	43	51	30,20	NNE	1	Cloudy.
	2	0	52	56	30,27	N	1	Rain.
14	8	0	39	52	30,28	W	1	Cloudy.
	2	0	49	56	30,20	N	1	Cloudy.
15	8	0	48	54	30,08	NNE	1	Cloudy.
	2	0	48	56	29,99	NE	1	Rain.
16	8	0	45	52	29,85	NE	1	Rain.
	2	0	46	56	29,95	E	1	Cloudy.

Rain this Month 0,553 Inches.

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1817	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Ser.	Weather.
Oct. 17	8	0	42	52	30,11		SE	1	Rain.
	2	0	49	56	30,08		SE	1	Cloudy.
18	8	0	42	52	29,95		SE	1	Rain.
	2	0	43	55	29,88		E	1	Cloudy.
19	8	0	44	52	29,93		E	1	Cloudy.
	2	0	45	48	29,92		NE	1	Cloudy.
20	8	0	43	50	29,95		NE	1	Rain.
	2	0	46	49	29,95		NE	1	Cloudy.
21	8	0	43	47	29,90		E	1	Cloudy, and hazy.
	2	0	48	49	29,87		SE	1	Rain.
22	8	0	44	49	29,81		NE	1	Cloudy and hazy.
	2	0	48	50	29,86		NE	1	Cloudy.
23	8	0	45	49	29,93		N	1	Cloudy.
	2	0	46	50	29,94		NE	1	Cloudy.
24	8	0	43	50	29,93		NE	1	Cloudy.
	2	0	44	51	29,91		ENE	1	Cloudy.
25	8	0	45	50	29,72		SW	1	Cloudy.
	2	0	43	50	29,76		SE	1	Cloudy.
26	8	0	44	49	29,86		SW	1	Cloudy.
	2	0	49	50	29,74		S	1	Fair.
27	8	0	40	48	29,71		SE	1	Hazy.
	2	0	48	52	29,51		N	1,2	Rain.
28	8	0	40	49	29,47		W	1	Fair.
	2	0	42	50	29,43		SW	1,2	Rain.
29	8	0	41	48	29,49		SW	1	Fine.
	2	0	44	48	29,54		NE	1	Fine.
30	8	0	49	50	29,29		SW	3	Rain.
	2	0	51	51	29,26		W by N	3	Fine.
31	8	0	47	49	29,56		SW	2	Rain.
	2	0	49	51	29,86		SE	1	Cloudy.

Rain this Month 0,553 Inches.

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for November, 1817.

1817	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Winds. Points.	Winds. Str.	Weather.
Nov. 1	8	0	39	49	30,27	W	1	Fine.
	2	0	53	52	30,37	W	1	Fine.
2	8	0	45	49	30,23	SW	2	Cloudy.
	2	0	53	50	30,19	W	1.2	Cloudy.
3	8	0	52	50	30,23	SW	1	Cloudy.
	2	0	50	52	30,21	SW	1	Cloudy.
4	8	0	49	52	30,10	SW	1	Cloudy.
	2	0	52	53	30,05	W	1	Cloudy.
5	8	0	48	53	29,97	S	1.2	Cloudy.
	2	0	52	61	29,95	S	1	Cloudy.
6	8	0	52	61	29,82	SW	1	Fine.
	2	0	53	57	29,92	S	1	Cloudy.
7	8	0	53	58	29,77	SW	1	Fine.
	2	0	57	63	29,63	SE	1	Cloudy.
8	8	0	52	59	29,51	SW	1	Fine.
	2	0	52	62	29,97	SW	2	Cloudy.
9	8	0	48	57	29,59	W	1	Fine.
	2	0	42	58	29,84	W	1	Fair.
10	8	0	48	56	29,96	W	1	Cloudy.
	2	0	53	60	29,99	SW	1	Cloudy.
11	8	0	50	58	29,76	SW	1	Fine.
	2	0	52	60	29,74	SW	1.2	Cloudy.
12	8	0	49	58	29,78	SSW	1	Cloudy.
	2	0	51	61	29,65	SW	1	Cloudy.
13	8	0	44	56	29,79	W	1	Fine.
	2	0	52	61	29,82	SW	1	Fine.
14	8	0	54	58	29,59	SW	1	Cloudy.
	2	0	54	62	29,50	SW	1	Rain.
15	8	0	53	59	29,33	S	2	Rain.
	2	0	51	61	26,52	W	1.2	Fine.

Rain this Month 1,110 Inches.

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for November, 1817.

1817	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Winds. Str.	Weather.
Nov. 16	8	0	48	58	29,90		WSW	1	Fine.
	2	0	50	58	29,97		SW	1	Cloudy.
17	8	0	53	57	30,20		W	1	Cloudy.
	2	0	57	60	30,23		W	1	Cloudy.
18	8	0	53	60	30,25		W	1	Fine.
	2	0	56	63	30,25		W	1	Cloudy.
19	8	0	42	57	30,46		W	1	Hazy.
	2	0	45	50	30,48		E	1	Fair.
20	8	0	40	53	30,50		Wby N	1	Foggy.
	2	0	50	59	30,10		E	1	Cloudy.
21	8	0	47	57	29,95		W	1	Cloudy.
	2	0	51	59	29,88		Wby N	1.2	Cloudy.
22	8	0	44	56	30,20		NNE	1.2	Hazy.
	2	0	47	59	30,18		NW	1	Cloudy.
23	8	0	43	55	30,13		W	1	Cloudy.
	2	0	49	54	30,08		NW	1	Cloudy.
24	8	0	44	54	29,89		W	1	Hazy.
	2	0	48	58	29,81		W	1	Rain.
25	8	0	37	52	29,82		W	1	Fine.
	2	0	42	56	30,04		W	1	Cloudy.
26	8	0	45	53	30,06		W	1	Cloudy.
	2	0	49	58	30,11		W	1	Cloudy.
27	8	0	47	54	30,14		W	1.2	Cloudy.
	2	0	52	58	30,16		SE	1	Cloudy.
28	8	0	47	55	30,14		W	1	Cloudy.
	2	0	46	56	30,05		W	1	Cloudy.
29	8	0	49	56	29 99		WSW	1	Cloudy.
	2	0	52	61	29,96		W	1	Cloudy.
30	8	0	52	57	29,96		W	1	Cloudy.
	2	0	53	57	29,90		W	1	Cloudy.

Rain this Month 1,110 Inches.

METEOROLOGICAL JOURNAL

for December, 1817.

1817	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Str.	Weather.
Dec. 1	8	0	54	57	29,85		WSW	1	Cloudy.
	2	0	54	60	29,75		WNW		Cloudy.
2	8	0	48	57	29,62		W	1	Cloudy.
	2	0	47	59	29,55		NW	1	Cloudy.
3	8	0	37	55	29,52		NW	1	Hazy.
	2	0	43	58	29,55		N	1	Fine.
4	8	0	36	54	29,94		N	1	Fine, rather hazy.
	2	0	38	57	29,96		N	1	Fair.
5	8	0	43	52	29,79		W	2	Cloudy.
	2	0	46	54	29,53		SW	2-3	Cloudy.
6	8	0	41	52	29,40		SSW	1	Dark and Cloudy.
	2	0	43	57	29,40		W	1	Fine.
7	8	0	37	51	29,41		W	1	Fine, rather hazy.
	2	0	42	51	29,38		W	1	Fine.
8	8	0	41	49	29,71		W	1	Cloudy.
	2	0	44	55	29,62		N	1	Rain.
9	8	0	39	51	29,13		NW	1-2	Hazy.
	2	0	39	55	29,22		NW	1	Fine.
10	8	0	34	50	29,27		WNW	1	Cloudy and hazy.
	2	0	37	54	29,37		NW	1	Fine.
11	8	0	30	47	29,57		W	1	Hazy.
	2	0	36	54	29,60		W	1	Fine.
12	8	0	27	48	29,72		W	1	Foggy.
	2	0	35	54	29,73		W	1	Hazy.
13	8	0	39	50	29,68		SW	1	Cloudy.
	2	0	42	53	29,65		SW	1	Cloudy.
14	8	0	42	50	29,53		S	1-2	Rain.
	2	0	46	50	29,50		S	1	Rain.
15	8	0	39	49	29,63		W	1	Fine.
	2	0	45	53	29,68		SW	1	Rain.
16	8	0	43	51	29,88		W	1	Rain.
	2	0	49	56	29,82		W	1	Cloudy.

Rain this Month 2,703 Inches.

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for December, 1817.

1817	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Dir.	Weather.
Dec. 17	8	0	39	52	29.60		W	1	Fine.
	2	0	43	57	29.71		WSW	1	Fine.
18	8	0	44	55	28,95		WNW	1	Hazy.
	2	0	46	56	28,91		W	1	Fine.
19	8	0	42	53	28,81		W	1	Cloudy.
	2	0	46	58	28,82		W	1	Cloudy.
20	8	0	42	53	29.36		NE	1	Hazy.
	2	0	43	55	29.57		N	1	Cloudy.
21	8	0	34	40	29.54		E	1	Hazy.
	2	0	37	48	29.51		E	1	Cloudy.
22	8	0	33	46	29.54		NE	1	Cloudy.
	2	0	36	52	29.53		NE	1	Fine.
23	8	0	36	43	29,55		N	1	Cloudy.
	2	0	34	48	29,55		SW	1	Hazy.
24	8	0	32	44	29.60		NW	1	Hazy.
	2	0	34	50	29.76		NE	1	Fine.
25	8	0	32	44	29.96		NE	1	Cloudy.
	2	0	35	45	29 97		N	1	Fair.
26	8	0	30	42	30,08		NNW	1	Hazy.
	2	0	33	48	30,12		NNW	1	Fair.
27	8	0	37	44	29,85		W	1	Cloudy.
	2	0	40	48	29,96		NW	1,2	Fair.
28	8	0	35	44	29,64		NW	1	Hazy.
	2	0	37	44	29,77		N	1	Fair.
29	8	0	28	42	30,13		NWbyW	1	Hazy.
	2	0	34	47	30,16		N	1	Fine.
30	8	0	36	42	30,00		W	1	Fair.
	2	0	42	51	29,97		W	1	Rain.
31	8	0	33	45	30,05			1	Foggy.
	2	0	31	45	30,05			1	Thick fog.

Rain this Month 2,703 Inches.

1817.	Thermometer without.			Thermometer within.			Barometer.*			Rain.†
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Inches.
January	53	28	41,5	57	37	49,1	30,61	28,80	29,66	2,154
February	53	37	45,2	60	49	53,7	30,53	29,47	29,94	0,875
March	55	30	43,8	61	47	53,8	30,50	28,87	29,84	1,057
April	59	34	47,0	64	48	55,9	30,56	29,75	30,22	0,120
May	65	42	51,6	64	54	58,0	30,29	29,23	29,74	1,698
June	81	47	62,8	77	55	64,7	30,32	29,38	29,85	1,860
July	70	54	60,8	68	57	62,9	30,04	29,17	29,81	1,412
August	68	50	57,8	69	60	64,1	30,08	29,00	29,72	1,458
September	69	50	57,3	69	53	60,8	30,19	29,22	29,94	0,300
October	59	39	48,0	65	47	52,1	30,30	29,08	29,89	0,553
November	57	37	49,2	63	49	56,7	30,50	29,33	30,04	1,110
December	54	27	39,0	60	40	50,7	30,16	28,62	29,61	2,703
Whole year			50,3			56,9			29,85	15,299

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

† The Society's Rain Gage is 114 feet above the same level, and 75 feet 6 inches above the surrounding ground.

Mean variation of the magnetic needle, June 1817, 24° 17′ West.